



Power Reveals

how clarity of responsibility shapes patterns of electoral accountability

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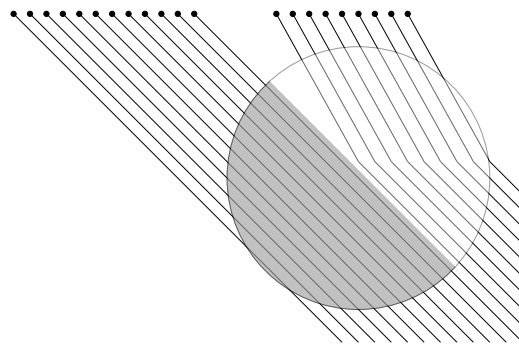
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Power Reveals

*how clarity of political responsibility shapes patterns
of electoral accountability*



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“[A]lthough the cliché says that power always corrupts, what is seldom said, but what is equally true, is that power always reveals. When a man is climbing, trying to persuade others to give him power, concealment is necessary: to hide traits that might make others reluctant to give him power, to hide also what he wants to do with that power; if men recognized the traits or realized the aims, they might refuse to give him what he wants. But as a man obtains more power, camouflage is less necessary. The curtain begins to rise. The revealing begins.”

– Robert A. Caro, *The Passage of Power*, 2013

Preface

This is an article-based dissertation. It seeks to answer the following overarching research question:

How does the extent of governments' power over policy outcomes shape patterns of electoral accountability?

It consists of a project frame (chapters 1-5) and the following four research articles (chapters 6-9):

- A. "Is the Relationship between Political Responsibility and Electoral Accountability Causal, Adaptive and Policy-specific?"
- B. "Clarity of Responsibility and Incumbent Behavior."
- C. "Incumbent Tenure Crowds out Economic Voting."
- D. "A Self-serving Bias in Attribution of Political Responsibility."

The research articles are self-contained and can therefore be read without reading the project frame. The goal of the project frame is threefold. First, to lay out the themes and approaches that cut across the different articles. Second, to summarize and distill the central findings from the research articles. Third, to discuss what implications these findings have for electoral accountability.

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Chapter 1

Introduction

“[O]ne of the weightiest objections to a plurality in the Executive [...] is, that it tends to conceal faults and destroy responsibility. [Blame] is shifted from one to another with so much dexterity, and under such plausible appearances, that the public opinion is left in suspense about the real author. [T]he plurality of the Executive [therefore] tends to deprive the people of the two greatest securities they can have for the faithful exercise of any delegated power, first, the restraints of public opinion, which lose their efficacy, [...] and, secondly, the opportunity of discovering with facility and clearness the misconduct of the persons they trust, in order either to their removal from office or to their actual punishment in cases which admit of it.”

Alexander Hamilton, *Federalist No. 70*.

Murals in Sienna

Quality of government is reflected in our everyday life. It resides in the fact that we can walk safely on the street, that the streets we walk safely on are well kept, and that the well-kept streets are populated by a prosperous people. One might argue that the importance of good government for the life of ordinary citizens is, in essence, the reason why studying politics is worthwhile. For if the actions of politicians did not have an important bearing on the life of these citizens, why should we care about the intricacies of politics at all?

The relationship between quality of life and quality of government has long been recognized by philosophers, political thinkers and artists. A particularly poignant example of the latter lies in the frescoes painted by Ambrogio Lorenzetti in the Palazzo Pubblico in Sienna, Italy. In these frescoes, Lorenzetti depicts Sienna in two different states of being: under good government and under bad government. Under good government, Sienna is ruled by justice and laws, with peace, prosperity and happiness as a result. Under bad government, Sienna is ruled by a tyrant, with war, famine and debauchery as a result. Under good government, Sienna seems light and inviting. Under bad government, Sienna is literally crumbling. Politics, in Lorenzetti's view of the world, makes all the difference.

Within the realm of modern political science, several theories have taken the same position as Lorenzetti, surmising that the quality of social and political outcomes reflects the quality of government (e.g., Halperin, Siegle and Weinstein, 2009). One such theory is retrospective voting. Basically, this theory argues that the competence of governing politicians 'rubs off' on society in transparent ways. Fiorina, an early scholar of retrospective voting, put it this way:

"[People] need not know the precise economic or foreign policies of the incumbent administration in order to see or feel the results of those policies. [...] If jobs have been lost, something is wrong. If sons have died in foreign rice paddies, something is wrong. If thugs make neighbourhoods unsafe, something is wrong. If polluters foul food, water, or air, something is wrong." Fiorina (1981, 5).

This view of the world is startlingly close to the one presented in Lorenzetti's murals: bad outcomes can be ascribed to bad (incumbent) governments, good outcomes to good government. Retrospective voting, however, is not really a theory about the extent to which the quality of government is reflected in society – it takes this as a

given. Instead, it posits that because quality of government is reflected in observable social and economic outcomes, people can make inferences about the quality of their government from the state of such outcomes (Fiorina, 1981). That is, at election time, when citizens in representative democracies have to decide whether they want to support the incumbent government, they can look back at whether important social and economic indicators have changed for the better or for the worse, inferring that, if they have changed for the better, this is probably because the incumbent government is competent. In this way, the voice of the people can be understood as an echo of the performance of the sitting government (Key, 1966).

From a normative standpoint, this is an appealing theory of how voters relate to their government, because it suggests that when citizens are given the opportunity to hold elected officials formally accountable through free and fair elections, they will tend to re-elect politicians that increase voter welfare and discard politicians that decrease voter welfare (Ashworth, 2012). This gives re-election minded politicians an incentive to pursue the type of policy outcomes that voters prefer. It also means that incumbent politicians who cannot or will not obtain the policy outcomes that voters prefer will be relieved of their duties. Ideally, retrospective voting thus implies that voters use elections to hold governments accountable, in the sense that they use retrospective performance to “discern whether governments are acting in their interest” and then make sure that “those incumbents who act in the best interest of citizens win reelection and those who do not lose them” (Manin, Przeworski and Stokes, 1999, 40). That is, under some circumstances, retrospective voting will create what Manin, Przeworski and Stokes calls “electoral accountability” (see also Ashworth, 2012; Hellwig and Samuels, 2008).

Retrospective voting is not just a normatively appealing theory; it is also, broadly speaking, descriptively accurate. A large number of studies have found that the electoral success of governing politicians is intimately related to the conditions they provide for their electorate (Healy and Malhotra, 2013). These studies have typically examined the relationship between economic conditions and incumbent support, finding that good economic conditions translate into electoral success for incumbents (Lewis-Beck and Stegmaier, 2013).¹ However, the basic pattern of voters punishing and rewarding politicians based on past performance is not limited to the economic

¹The prevalence of studies that examine economic performance have lead some to use the term “economic voting” instead of retrospective voting. This is largely a question of semantics. The project frame of the dissertation opts for the term retrospective voting, because, in some cases, the dissertation will examine types of retrospective performance that are not directly about the state of the economy (e.g., public service provision). In the research articles that only examine retrospective economic performance, I use the term economic voting.

sphere (for some recent studies that go beyond economic conditions, see Burnett and Kogan, 2017; Healy and Malhotra, 2010; Howell, 2005; Tilley and Hobolt, 2011).

Imperfect Unmasking

Retrospective voting implies that politicians are held to account for increases or decreases in voter welfare; however, most people would probably recognize that the government's ability to affect voter welfare is limited.

One account of the limitations politicians face in controlling the welfare of their subjects is found in the writings of Niccolo Machiavelli. In his historical analysis, Machiavelli insisted that he had found systematic differences in the fate of Italy's city states based on the *virtú* of their princes, i.e., their competence or quality. Even so, he recognized that the fate of a given prince was not determined by the *virtú* alone. An equally strong force was the whimsy of the goddess of luck *fortuna* (Machiavelli, 1975 [1532]). By *fortuna* Machiavelli meant something along the lines of the unalterable circumstances the prince had to work under. While princes with particularly strong *virtú* might cancel out the woes of *fortuna* for a short while, Machiavelli suggested that the fate of any prince was inextricably linked to both forces; to their inherent quality and to the circumstances under which they governed. Put differently, even though the quality of the government (or prince) is reflected in the quality of the life the government provides for its citizens, this reflection is not perfect.

So exactly how important is the quality of government in producing social outcomes? Machiavelli suggested that, on average, *virtú* was about as important as *fortuna*, with the latter perhaps being slightly more important (Machiavelli, 1975 [1532], 166). If one looks at contemporary representative democracies, however, it is clear that any such average assessment makes little sense, because polities vary greatly in the extent to which the quality of executive governments translates into policy outcomes.

To see this, contrast a British Prime Minister facing some kind of crisis which requires passing legislation with an American President facing a similar crisis. The British Prime Minister, who operates in a textbook majoritarian system, can simply devise the legislation they deem appropriate and rely on their majority in the House of Commons to pass it. If the American President wants to pass a similar piece of legislation, they will need to compromise with a majority of legislators in both the House of Representatives and in the Senate, who might be interested in seeing the President's plan fail. This means that even if the President and the Prime Minister

have the same *virtú*, it is only the latter who will be able to easily translate this *virtú* into good legislation without help from the goddess of luck (i.e., co-operation from a combative legislative branch).

Other factors can also play a role in shaping the extent to which the quality of government is translated into policy outcomes. For instance, many economists would argue that politicians in more open economies, where capital and goods flow freely in and out of the country, will be more constrained with respect to what fiscal and monetary policies are feasible, and more generally, will have a harder time regulating the economy. These constraints attenuate the relationship between the quality of government and the quality of the economy, as even the most skilled economic manager will struggle to stave off the adverse effects of a global recession in an open economy, whereas even exceptionally incompetent economic managers will see economic outcomes improve during a global economic boom.

These examples illustrate that the reflection of government quality in policy outcomes, that is imagined in Lorenzetti's frescoes, as well as in the theory of retrospective voting, is not just imperfect, it also varies with the political and economic context. This variation in the extent to which the quality of government is reflected in policy outcomes can be conceptualized in terms of the *power over policy outcomes* the particular political and economic context assigns to the government. While power is a contested concept, one can reasonably define it as "A has power over B to the extent that he can get B to do something that B would not otherwise do" (Dahl, 1957, 202-203), and in this context it makes sense to say that differences in the extent to which the government can shape policy outcomes can be defined as the extent to which the government has power over policy outcomes.

Following this, we can say that typical majoritarian parliamentary systems gives the incumbent more power over legislative outcomes than the typical presidential system does, and that incumbent governments have less power over economic outcomes in open economies than they have in closed economies. More generally, we can say that the more power a government has over policy outcomes, the more the quality of these policy outcomes will reflect the government's quality. Thus, less power over a policy outcome muddles the extent to which politicians are responsible for this outcome. More power, on the other hand, reveals.

Research Question

It seems intuitive that variation, in the extent to which incumbent politicians have power over important policy outcomes is relevant in the context of retrospective voting. If voters hold their government accountable for changes in voter welfare in contexts where the government has limited power over voter welfare, they risk electorally rewarding and/or punishing the “wrong” politicians. In particular, they risk treating an unlucky high-quality government the same way they do a lucky low-quality government. This naturally puts the normative appeal of retrospective voting into question, because it is no longer clear that governments will be held accountable for how they perform in office (Achen and Bartels, 2016).

This tension between the variation in governments’ power over policy outcomes and the extent to which retrospective voting will produce effective electoral accountability motivates the following research question.

How does the extent of governments’ power over policy outcomes shape patterns of electoral accountability?

As already established, the obvious answer to this question is that when a government’s power over policy outcomes is more limited, the prospects for effective electoral accountability will be limited. However, seemingly obvious answers are often worth exploring, and while this dissertation will to some extent find that the obvious answer is also the right one, it will also find that in some cases the obvious answer needs to be amended, if not discarded entirely.

Answering this particular research question is important, because it tells us something about the conditions under which representative government will lead to good government. That is, if the dissertation finds that diluting governments’ power over policy outcomes is harmful to electoral accountability, then it provides some normative justification for a specific set of political institutions (i.e., those who consolidate the political power of central governments). Further, exploring the consequences that governments’ power over policy outcomes have for electoral accountability will give us some insight into the pressures that governments face when it comes to delivering high quality policy outcomes, and about voters’ abilities to draw appropriate inferences about the quality of their government. These insights are valuable if one wants to form a realistic ‘mental picture’ about the inner workings of contemporary representative democracies. In essence, this dissertation attempts to answer the call made by Healy and Malhotra (2013, 287) for a “rich and accurate

model of voter behavior” that accounts for “the conditions under which retrospective voting achieves effective democratic accountability and when it fails to do so.”

Grappling with the consequences that the dilution of political power has for electoral accountability is also timely. For one, the value of increased economic and political globalization has become a hotly contested topic in most Western countries, something which has only been spurred on by the anti-trade sentiment of American P Donald Trump, and by Great Britain’s decision to secede from the European Union. One important implication of globalization is that national governments’ power becomes more diluted (Hellwig and Samuels, 2007; Hellwig, 2001), and by exploring what implications this dilution has for electoral accountability, this dissertation can contribute to this debate. In addition to this, the status of electoral accountability is also contested within the political science literature. In a recent book, veteran political scientists Achen and Bartels (2016) argue that representative governments are essentially never held accountable by their electorates, because no electorate has the capacity to rationally evaluate the state of policy outcomes or the extent to which their government is responsible for these outcomes. The premise of this dissertation naturally pushes back against this conclusion, since it wants to investigate differences in patterns of electoral accountability, and in order for this to be interesting, one needs to presume the possible existence of electoral accountability. Even so, the dissertation could, in principle, end up concluding that governments’ power over policy outcomes has no effect on electoral accountability, because it is impossible to find any evidence of accountability.

Answering the Research Question

This dissertation does not represent the first attempt to try and understand the relationship between governments’ power over policy outcomes and electoral accountability, and while the next chapter will detail the received wisdom from the researchers who have tackled this relationship in the past, one strand of research deserves to be mentioned up front: the literature on clarity of responsibility. First developed by Powell and Whitten (1993), the clarity of responsibility literature asserts that voters’ propensity to hold governments accountable for economic performance “will strongly reflect the nature of policymaking in the society and the coherence and control the government can exert over that policy” (Powell and Whitten, 1993, 398; for some earlier formulations of the same sentiment, see Lewis-Beck, 1990; Paldam, 1991). A plethora of studies have found that this argument is, broadly speaking, correct (for a recent review, see Silva and Whitten, 2017). If a government has coherent control

over policy, then this government is more likely to be held accountable for policy outcomes.

Since the coherent control over policy that Powell and Whitten (1993) speak of is arguably similar to the notion of “governments’ power over policy outcomes” introduced above, these findings speak directly to the research question.² Accordingly, the dissertation will use the clarity of responsibility literature as a stepping stone, building directly on the findings from this literature. In particular, the dissertation will try to advance our knowledge in four different ways.

First, it will try to characterize the relationship between governments’ power over policy outcomes and electoral accountability more fully. While the clarity of responsibility literature has identified an aggregate correlation between the extent to which governments have power over policy outcomes and the extent to which governments are held accountable for policy outcomes, several aspects of the relationship remain in question. Does the identified correlation, for instance, reflect a causal relationship? If governments’ power over policy outcomes changes, does that mean electoral accountability follows suit? If so, how long does it take? Does the change in electoral accountability follow instantaneously or is the relationship more sticky? And is the relationship policy specific? That is, if a government gets more power over a specific policy outcome, will voters only hold them more accountable for this specific outcome? Or does voter attribution work at a higher level of analysis? In its effort to pin down the characteristics of the relationship between governments’ power over policy outcomes and electoral accountability, this dissertation is among the first provide answers to all of these questions.

Second, it will look at what implications the relationship between governments’ power over policy outcomes and electoral accountability have for the behavior of office holders. Incumbent behavior has typically not been studied in the clarity of responsibility literature, however, if centralized power breeds electoral accountability, then centralized power might also affect the composition and incentives of incumbents. In particular, the dissertation explores whether increasing governments’

²This dissertation uses the term ‘clarity of responsibility’ interchangeably with ‘power over policy outcomes’. The two concepts might not sound that similar. In particular, clarity of responsibility seems to be a concept that belongs at the ‘micro’-level, mapping whether individual voters perceive the government to be more or less clearly responsible for economic outcomes. However, if one looks at Powell and Whitten’s original definition (cf. above), and later iterations (e.g., Hobolt, Tilley and Banducci, 2013; Nadeau, Niemi and Yoshinaka, 2002; Whitten and Palmer, 1999), it is clear that the clarity of responsibility concept maps onto governments, not individuals, indexing the extent to which governments have power over policy outcomes in specific contexts. This becomes even more clear if one looks at the empirical indicators of high and low levels of clarity of responsibility that previous research works have used (cf. chapter 2).

power over policy outcomes makes it more likely that incumbents obtain the type of policy outcomes voters prefer.

Third, it examines whether and how incumbents' time in office affects patterns of electoral accountability. Time in office is interesting to single out for two reasons. First, it has only received modest attention in the existing literature (Singer and Carlin, 2013). Second, it might provide a corrective to the idea that if a government gets more power over policy outcomes it will also be held more electorally accountable for these policy outcomes. In particular, there might be another force at work which washes out, or alternatively dominates, the increases in power over policy outcomes that time in office almost surely entails: learning. As such, as a government's time in office increases, voters might learn something about this government, and this might lead them to hold the government less accountable for the state of important policy outcomes. If, for instance, a government has produced good policy outcomes seven years in a row and then have a bad year, voters might be more inclined to think that this bad year is a result of a single bad encounter with the goddess of luck. Conversely, a government that is just starting out might not be given the benefit of the doubt if it starts out with a bad year, even if voters know that new governments generally have less power to shape outcomes than more experienced governments. Against this backdrop, this dissertation provides the most thorough empirical investigation of the relationship between retrospective voting and time in office to date.

Fourth, going beyond the clarity of responsibility literature, the dissertation looks at how voters hold their government accountable for policy outcomes that are partly the result of government intervention and partly a result of individual voters' own actions (i.e., one's own employment situation). In particular, following psychological theories about self-servingness, the dissertation explores whether voters are more likely to hold the government accountable for such outcomes if they are undesirable as opposed to desirable.

The dissertation will be organized around these four focus points that each, in their own way, advance the research frontier. It is important to note that while these four focus points all follow from the research question, they are by no means the only focus points one could have arrived at. They represent areas of research that are necessary to touch upon if one want to answer the research question, but they are not sufficient. Put differently, even though this dissertation presents the scholarly literature with a more qualified answer to the question of how governments' power over policy outcomes shape patterns of electoral accountability, it still leaves plenty of room for future research.

Organization of the Dissertation

This is an article-based dissertation, and therefore each of the four focus points listed above will be dealt with in individual research articles. Table 1.1 presents an overview of the different articles. The theoretical discussions and empirical evidence presented in these articles will be the heart of the dissertation, and it will be here that the relation and contribution to the existing literature will be the most obvious.

Table 1.1 Overview of the Articles in the Dissertation

Short title	Title	Chapter	Status
A: Responsibility and Accountability	<i>Is the Relationship between Political Responsibility and Electoral Accountability Causal, Adaptive and Policy-specific?</i>	6	Under Review
B: Incumbent Behavior*	<i>Clarity of Responsibility and Incumbent Behavior</i>	7	Working Paper
C: Incumbent Tenure	<i>Incumbent Tenure Crowds out Economic Voting</i>	8	Under Review
D: Self-servingness	<i>A Self-serving Bias in Attribution of Political Responsibility</i>	9	Under Review

*Co-authored with Asmus Leth Olsen.

Before moving on with the articles, however, the dissertation will offer some general remarks. In particular, the next chapter will detail the nature and limitations of existing efforts to explain how electoral accountability works, and delineate how the dissertation will try to expand upon these efforts in the individual articles. The third chapter looks at some common methodological problems the articles face in terms of research design, and detail how the articles attempt to overcome these problems. The fourth chapter summarizes the results from the different articles. The fifth chapter revisits the research question, and discusses what has been learned from the different articles and what implications these lessons have. Chapters six through nine contain the different research articles.

Chapter 2

Theory, Previous Work, and Unanswered Questions

This chapter goes into detail regarding the dissertation's key concepts, its relation to the existing literature and the contributions it seeks to make. The chapter is split into three parts. The first part of the chapter focuses on the dissertation's main dependent variable: electoral accountability. Expanding on the arguments presented in chapter 1, the theoretical and empirical relationship between electoral accountability and retrospective voting is discussed. In particular, I argue that retrospective voting can be used as an (imperfect) indicator of electoral accountability. The second part of the chapter focuses on the main independent variable, power over policy outcomes, discussing the existing attempts to understand how this variable shapes patterns of electoral accountability. In doing so, the chapter draws heavily on the clarity of responsibility literature which was briefly presented in the introduction. The third and final part of the chapter details how this dissertation advances our understanding of the relationship between governments' power over policy outcomes and electoral accountability, detailing the dissertation's theoretical, empirical and methodological contributions.

Electoral Accountability and Retrospective Voting

The key dependent variable in this dissertation is electoral accountability. Electoral accountability is a somewhat contested concept, however most would agree that it is fundamentally about the extent to which voters can use elections to constrain politicians so that they act in the voters' best interests (cf. Franklin, Soroka and Wlezien, 2014; Manin, Przeworski and Stokes, 1999). This notion of electoral accountability has been a central part of normative and empirical democratic theory at least since *the Federalist Papers* (Hamilton, Madison and Jay 2009 [1788]). Arguably, the key question in the political science literature on electoral accountability has been: how can and do voters use elections to constrain political action so that these actions are in the voters' best interest? (For a recent review of this literature, see Ashworth, 2012). One answer to this question has been found in the extensive literature on retrospective voting (e.g., Lewis-Beck and Stegmaier, 2013).

As mentioned in the introduction, the literature on retrospective voting suggests that voters base their decision to reelect governing politicians on the quality of key policy outcomes, such as the economic situation (Healy and Malhotra, 2013). The retrospective voting literature, also known as the economic or performance voting literature, is one of the largest and most successful empirical literatures within political science, and there is strong evidence of retrospective voting in the US (Fiorina, 1981; Kramer, 1971; Nadeau and Lewis-Beck, 2001), Western Europe (Duch and Stevenson, 2008; Lewis-Beck, 1990), Eastern Europe (Tucker, 2006), and Latin America (Singer, 2013a).¹

There are two reasons why retrospective voting behavior might lead to electoral accountability (i.e., to voters constraining government's actions so that these actions are in the voters' best interests). The first is that retrospective voters will tend to select 'good' types of governments, which produce the type of policy outcomes voters prefer. To see this, imagine that an incumbent government is incompetent and therefore tends to produce poor policy outcomes (i.e., outcomes voters do not like). If such an incumbent faces retrospective voters, who only reelect incumbents that produce good outcomes, then this incumbent's time in office will be cut short. Conversely, a competent incumbent, which produces policy outcomes voters like, will tend to be reelected. This process is often referred to as a *selection* mechanism,

¹There are a few detractors to the view that retrospective voting is widespread (e.g., Cheibub and Przeworski, 1999; Evans and Chzhen, 2016; Van der Brug, Van der Eijk and Franklin, 2007), however, these run counter to the scholarly consensus reported in a number of recent review articles (i.e., Ashworth, 2012; Healy and Malhotra, 2013; Lewis-Beck and Stegmaier, 2013).

because the selection of a particular type of incumbent is what constrains incumbent behavior (e.g., Duch and Stevenson, 2008; Fearon, 1999). The other reason is that when incumbent governments face retrospective voters, they will work harder to produce the kind of policy outcomes voters prefer. To see this, we need to make the relatively weak assumption that it requires a good deal of effort to shape policy outcomes. If this is the case, incumbents will need to be provided with some kind of incentive in order to exert this effort. Retrospective voters create this incentive by making re-election contingent on providing a specific set of policy outcomes. This process is often referred to as a *sanctioning* mechanism, because the threat of electoral sanctions constrain incumbent behavior (e.g., Barro, 1973; Besley, 2007; Ferejohn, 1986).

These selection and sanctioning mechanisms provide a strong theoretical link between retrospective voting and effective electoral accountability. Empirically, things are, as always, a bit more murky. While some researchers have shown that the sanctioning and selection mechanisms described above do constrain incumbent behavior in predictable ways (Alt, De Mesquita and Rose, 2011; Besley and Case, 1995a; De Janvry, Finan and Sadoulet, 2012; Ferraz and Finan, 2011), others have remained skeptical about the idea that retrospective voting will, in fact, lead to this type of selection and sanctioning.

For one, some scholars have argued that while voters do punish and reward incumbents based on the quality of policy outcomes, voters' assessments of these outcomes are regularly distorted by perceptual biases. Healy and Lenz (2014), for instance, show that voters tend to over-emphasize recent trends in economic outcomes when evaluating the state of the economy (see also Achen and Bartels, 2016; Healy and Malhotra, 2009; Huber, Hill and Lenz, 2012). Others have found that partisans adjust their views on the economy based on whether their preferred party is in power (e.g., Evans and Andersen, 2006), and that all voters tend to over-emphasize negative outcomes (e.g., Soroka, 2006).

Along the same lines, some have argued that voters have a hard time attributing responsibility for policy outcomes in a reasonable way. As such, a number of studies have found that voters punish and reward incumbent governments for outcomes that the government has no control over (Achen, 2012; Busby, Druckman and Fredendall, 2017; Healy, Malhotra and Mo, 2010; Sances, 2017; for a criticism of some of these findings, see Fowler and Hall, 2016; Fowler and Montagnes, 2015). Also, some have found that partisans selectively attribute responsibility, letting their preferred party

off the hook for poor policy outcomes when that party is in government (Bisgaard, 2015; Malhotra and Kuo, 2008; Tilley and Hobolt, 2011).

These perceptual and attributional biases inhibit both selection and sanctioning. It inhibits the selection mechanism in that governments are not selected based on their actual record, but rather based on voters' *misperception* of this record. It inhibits the sanctioning mechanism because it provides incentives for politicians to provide policy outcomes that voters are not really interested in. For instance, if voters are myopic, focusing more on recent policy outcomes, politicians will have an incentive to divert resources from the beginning of the election period towards the end of the election period (Kayser, 2005; Smith, 2003; Tufte, 1980).

There is no scholarly consensus about the extent to which these psychological biases distort the translation of retrospective voting into effective electoral accountability. In their recent book, *Democracy for Realists*, Achen and Bartels (2016, 143) conclude that the type retrospective voting that people actually engage in "is very unlikely to provide much in the way of effective accountability". On the other hand, Fowler and Hall (2016, 26) conclude that "recent concerns about voter competence [...] are overblown". A balanced view is probably be that while misperceptions and misattributions attenuate the relationship between retrospective voting and electoral accountability, it does not undo it completely. This is also the conclusion Healy and Malhotra (2013, 288) settle on in their recent review of the retrospective voting literature.

Taking this balanced view has two implication for how this dissertation will go about studying the relationship between government's power over policy outcomes and electoral accountability. First, the dissertation will conceptualize electoral accountability in terms of retrospective voting, interpreting more retrospective voting as an indicator of more electoral accountability. Second, while retrospective voting will be interpreted as an indicator of electoral accountability, it will not be interpreted as a sufficient indicator. As such, to the extent that the dissertation finds signs of retrospective voting, it will try to tease out whether this reflects effective electoral accountability and the extent to which it reflects misperceptions or misattributions.

Why Not Look at Issue Voting?

In terms of strategies voters can use to hold politicians electorally accountable, the main alternative to retrospective voting is issue voting.² Whereas retrospective voting

²This dissertation is not alone in arriving at this dichotomy (cf. Achen and Bartels, 2016; Lenz, 2012).

is premised on voters responding to the performance of politicians, so that higher-performing politicians are elected, issue voting is premised on voters responding to the election programs put forward by the politicians running for office, so that politicians who run on more popular policy platforms are elected (e.g., Downs, 1957). So, do voters use such an issue voting strategy? And how does it compare to the retrospective voting strategy?

On a theoretical level, the issue voting strategy is challenged by the commitment problem that politicians face when communicating their issue positions to voters. No matter what type of policy the politicians prefer when they are in office, politicians will have an electoral incentive to tell voters that they will enact voters' preferred type of policy. If politicians want to pursue unpopular policies while in office, they will have an incentive to lie to the voters (there is a "pooling" equilibrium, cf. Persson and Tabellini, 2002). Note that a similar commitment problem does not exist in the context of retrospective voting, because voters base their decision on what politicians have done (i.e., the policy outcomes they have created), not on what politicians say they have done (although see Bisgaard and Slothuus, 2017).

On an empirical level, it is not clear to what extent this commitment problem is a factor. Politicians sometimes renege on their campaign promises, but more often than not, they keep them (Pétry and Collette, 2009; Thomson et al., 2017). Even so, there are other empirical challenges to the policy/issue voting strategy. First, a large body of literature suggests that voters' knowledge of parties' positions on important issues are limited (Andersen, Tilley and Heath, 2005; Converse, 1962; Zaller, 1992). Second, to the extent that voters do know politicians' issue positions, these do not tend to have an independent effect on their decision to support a particular party. Lenz (2012), for instance, shows that if people learn that their preferred party holds an issue position that they disagree with, voters will change their position on the issue, not who they intend to vote for. As such, politicians are, generally speaking, not constrained by the voters when it comes to taking issue positions (see also Abramowitz, 1978; Broockman and Butler, 2017; Jacoby, 1988; Minozzi et al., 2015; Slothuus, 2010).

There are some exceptions to this rule. For one, Tesler (2015) finds that some voters will change their vote if a party takes an issue position that reflects long-standing group affiliations or other affectively charged predispositions (e.g., gay rights). Similarly, Hall (2015) finds that if a party fields a very ideologically extreme candidate this hurts their electoral prospects. However, outside of ideologically extreme candidates and issues that are tied to some deeply held and widely-shared predisposition, there

is only little persuasive evidence that voters use an issue voting strategy to hold politicians electorally accountable.

As mentioned above, some argue that voters are just as ignorant and irrational when it comes to drawing inferences from and about the state of important policy outcomes as they are when it comes to issue positions (Achen and Bartels, 2016). There are reasons to doubt this assertion. As discussed in the introduction, voters can learn about policy outcomes without much effort. As Fiorina (1981, 5) puts it: “[Even uniformed voters] typically have one comparatively hard bit of data: they know what life has been like during the incumbent’s administration”. The same sentiment is mirrored by Popkin (1991, 24) who says that “[P]olitical information is acquired while making individual economic decisions and navigating daily life: shoppers learn about inflation of retail prices; home buyers find out about mortgage-loan interest rates; owners of stock follow the Dow-Jones averages”(see also Stevenson and Duch, 2013). In this way, learning of policy outcomes can happen inadvertently. This stands in contrast to knowledge of issue positions, which people only learn from seeking out the information themselves or from watching political news; actions that require some more effort on the part of the voter.

In addition to this more conceptual point, the causal evidence for an independent effect of retrospective performance is also much stronger than the evidence for an independent effect of issue positions. For instance, in the study by Lenz (2012) mentioned above, he also analyses the effect of economic conditions. He finds that if someone’s preferred party is in power, and they learn that the economy is doing poorly, they will support one of the parties out of power. This is the opposite of what was found for issue positions, where party choice remained stable while issue positions changed. Others have found similar results using both quasi-experimental (Healy and Malhotra, 2010) and experimental designs (Simonovits, 2015).

This dissertation does not engage further with the issue voting strategy, asserting, based on the previous literature and the considerations laid out above, that the most important way that voters hold politicians electorally accountable is by using a retrospective voting strategy.³

³One of the research articles does engage with local income tax policy, however, even this article does not look at the proposed income tax policy but rather the enacted policy.

Clarity of Responsibility

The goal of this dissertation is to explore how governments' power over policy outcomes shapes patterns of electoral accountability. As mentioned in the introduction, the first systematic analysis of this question was conducted by Powell and Whitten (1993). In this seminal article, Powell and Whitten tried to explain why retrospective economic voting varied from one country to another. They came to investigate governments' power over (economic) policy outcomes because they asserted that "the critical linkage of the voter's assignment of responsibility to the government is not merely an individual-level idiosyncrasy or rationalization." Instead, they argued that assignment of responsibility would "reflect the nature of policymaking in the society and the coherence and control the government can exert over that policy", so that "[t]he greater the perceived unified control of policymaking by the incumbent government, the more likely is the citizen to assign responsibility for economic and political outcomes to the incumbents" (Powell and Whitten, 1993, 398). Powell and Whitten called this the 'clarity of responsibility' hypothesis, signifying that in elections where the incumbent government has coherent control of the economy, they are more clearly responsible.

Powell and Whitten made no additional theoretical arguments for this assertion, nor did they explicitly link clarity of responsibility to electoral accountability. Subsequent research has made amends on both counts, developing a more rigorous theoretical justification with important implications for electoral accountability. The most prominent example of this is Duch and Stevenson (2008, 2005, 2006, 2010) who developed and tested a theoretical model which explains why clarity of responsibility moderates retrospective voting, and in, turn electoral accountability. In particular, their model finds that it is rational for voters to rely less on the economic situation when deciding whether to vote for the incumbent government if the state of the economy situation is less sensitive on decisions made by the government. Their model also shows that this has a detrimental effect on electoral accountability, because as the effect of the incumbent government's quality on the economy diminishes, it becomes harder for voters to sort 'high-quality' incumbents from 'low-quality' incumbents on election day (cf. the selection mechanism described above).

Duch and Stevenson are not alone in concluding that clarity of responsibility moderates electoral accountability. In Besley's (2006) canonical presentation of how retrospective voting interacts with selection and sanctioning mechanisms in different agency models, he argues that if there is a "problem of untangling responsibilities

for outcomes” then “this may be expected to weaken the mapping from outcomes to re-election decisions by individuals” (Besley, 2006, 105; see also Achen and Bartels, 2016; Persson and Tabellini, 2002, chap. 4).

In light of the already discussed criticisms levied against the normative potential of retrospective voting, these theoretical models might sound implausible. If voters have a hard time making meaningful assessments about the quality and causes of policy outcomes, how can they be expected to make meaningful assessments about the extent to which the government controls policy outcomes in different contexts? In spite of this, a number of studies have found that voters do in fact view their government as more responsible for economic and other policy outcomes when objective conditions – such as economic and political institutions – assign more power over policy outcomes to the government (Alcañiz and Hellwig, 2011; Duch and Stevenson, 2008; Hobolt and Tilley, 2014; Rudolph, 2003a). While this finding can be hard to reconcile with studies which show that voters have substantial informational shortcomings, it is important to note that the finding does not necessarily reflect voters actively seeking out information about the relative power their government has over policy outcomes. Instead, it might be explained in terms of political elites, such as the media, focusing more on the types of policy outcomes that governments have more responsibility for in public debates, and voters inferring from this that these outcomes are subject to political manipulation (for evidence of this basic mechanism, see Hart, 2016).

Empirical Evidence for the Clarity of Responsibility Hypothesis

Inspired by the deviation into majoritarian and consensus governments laid out by Lijphart (1984), Powell and Whitten operationalized clarity of responsibility in terms of how the legislature was organized (i.e., party cohesion, strong legislative committees, bicameral opposition) and how the government was organized (i.e., minority government, number of government parties). Based on these different indicators they created an index, finding that economic conditions, such as GDP per capita growth and inflation, had a larger effect on incumbent support in countries that had a high score on their clarity of responsibility index.

Most empirical research on clarity of responsibility has basically used the same approach as Powell and Whitten, estimating differences in the levels of retrospective voting across contexts that assign different degrees of power over policy outcomes to incumbent governments (for an extensive and recent review, see Silva and Whitten, 2017). These studies have replicated and refined Powell and Whitten’s findings,

demonstrating the relevance of alternative indicators of clarity of responsibility, for instance the effective number of parties (Nadeau, Niemi and Yoshinaka, 2002), volatility (Bengtsson, 2004), multi-level government (Anderson, 2006; León, 2011) and globalization (Duch and Stevenson, 2008; Hellwig and Samuels, 2008; Hellwig, 2001; Lobo and Lewis-Beck, 2012), and that some of the indicators used by Powell and Whitten were probably not that important (Dassonneville and Lewis-Beck, 2017; Hobolt, Tilley and Banducci, 2013; Royed, Leyden and Borrelli, 2000).

Almost all empirical tests of the clarity of responsibility hypothesis focus on economic retrospective voting, tracking the extent to which clarity of responsibility moderates the effect of economic conditions on support for the incumbent government; however, a few studies have looked at other policy outcomes as well, finding similar results. Schwindt-Bayer and Tavits (2016), for instance, look at corruption, finding that incumbents are punished more severely for instances of corruption when clarity of responsibility is high (see also Hobolt, Tilley and Banducci, 2013, who study clarity of responsibility in relation to health care outcomes).

In sum, *a lot* of studies have found that if an economic or political institution assigns more power over policy outcomes to the incumbent government, then the presence of said institution will coincide with more retrospective voting. This suggests that the clarity of responsibility hypothesis is basically right; however, some more recent findings complicate this general conclusion.

In particular, some studies find that voters hold the government accountable for outcomes politicians are not responsible for at all. Most notably, Healy, Malhotra and Mo (2010) find that the results of local college football games have an effect on support for the incumbent in US elections (see also Busby, Druckman and Fredendall, 2017). This is hard to square with the clarity of responsibility hypothesis in the sense that the government's power over the result of college football games is non-existent. Similarly, Sances (2017) finds that voters hold the US president accountable for tax increases which are enacted by way of local referenda, and Wolfers (2007) finds that voters hold local incumbents accountable for economic shocks that can be attributed to changes in the price of oil (see also Leigh, 2009).

While these results cannot be explained using the clarity of responsibility hypothesis, they are not necessarily at odds with it either. To see this, imagine that retrospective voting is driven by two separate forces. One is that voters decide to hold the incumbent government accountable for the things that they believe the incumbent is responsible for. The other is that when something bad happens, affective contagion (or some other psychological mechanism) lead voters to like the

incumbent less. To the extent that clarity of responsibility moderates the strength of the former force (i.e., cognizant assignment of responsibility) but not the strength of the latter (affective contagion), we would expect clarity of responsibility to moderate retrospective voting, and for policy outcomes which are outside the government's sphere of influence to have some effect as well. This is the exact pattern identified by the existing literature.

Another challenge to the evidence for the clarity of responsibility hypothesis comes from Parker-Stephen (2013). He argues that clarity of responsibility does not just moderate retrospective voting, it also moderates the strength of partisan motivated reasoning, so that partisans are more inclined to let their partisanship inform their evaluations of policy outcomes in a high clarity context. The proposed explanation for this is that as the clarity of responsibility of some policy outcomes increases, the status of this policy outcome becomes more central to evaluating government performance, giving party identifiers a clearer incentive to engage in directional motivated reasoning. If this is true, clarity of responsibility does not necessarily increase or decrease the level of retrospective voting, it simply increases the appearance of retrospective voting, because it polarizes government and opposition partisans' perceptions of retrospective performance.

Parker-Stephen's argument provides an important corrective to the clarity of responsibility literature; however, as he himself recognizes, it has limited implications. For one, it is hard to empirically disentangle increased partisan polarization from increased retrospective voting, as both are identified by looking at differences in evaluations of retrospective performance across government and opposition supporters. In addition to this, Parker-Stephen's argument only applies to studies that use voters' perceptions of policy outcomes to identify levels of retrospective voting. Several studies of clarity of responsibility, including Powell and Whitten's original study, use objective indicators such as inflation, GDP growth and unemployment rates. Government and opposition partisans can obviously not 'polarize' on these objective economic indicators.

Some Important, Yet Unanswered, Questions

Having laid out the existing evidence on how governments' power over policy outcomes shapes patterns of electoral accountability, the next task will be identifying some pressure points where this dissertation can move the literature forward. In particular, I lay out, in turn, four different questions that the existing literature has

left unanswered, and which this dissertation will try to answer. The four different questions map onto the four different research articles that are presented in the final four chapters of the dissertation.⁴

A: What characterizes the relationship between governments' power over policy outcomes and electoral accountability? While the existing literature has convincingly shown that there is an aggregate level correlation between governments' power over policy outcomes and electoral accountability, it has left several aspects of the relationship under-illuminated. In particular, it is not clear whether the relationship is causal, adaptive or policy-specific.

- It is not clear whether it is *causal*, because the existing literature relies almost entirely on cross-country observational correlations between the presence of specific institutions and the extent to which the incumbent government is held accountable for the quality of policy outcomes, and since institutions are 'unnatural experiments' (Besley and Case, 2000), this correlation could potentially be spurious (more on this in chapter 3).
- It is not clear whether the relationship is *adaptive*, because previous research has focused on institutional differences that change slowly, making it impossible to determine whether the aggregate level relationship reflects a long-term process where accountability slowly adjusts to the changes in governments' power over policy outcomes (i.e., adapts slowly), or a short-term process in which voters instantly adjust their behavior (i.e., adapts more quickly).
- It is not clear whether the relationship is *policy-specific*, because the previous literature has generally only examined differences in clarity of responsibility for policy outcomes as such, not clarity of responsibility for specific policy outcomes (although see Arceneaux, 2006; Ruder et al., 2014). As such, we do not know whether increasing political responsibility for an individual policy outcome leads voters to hold the incumbent more accountable for only this outcome, or whether increasing responsibility for an individual policy outcome lead voters to hold incumbents more accountable for other outcomes as well.

Getting at these features of the relationship between governments' power over policy outcomes and electoral accountability is important, because they all tell us something about the extent to which incumbent governments are held exclusively

⁴Parts of what follows in this section are taken more or less directly from the different research articles.

electorally accountable for the outcomes they had a hand in shaping, and, in turn, how adept voters are at identifying and (re)electing governments who are able and willing to produce the type of policy outcomes they prefer.

B: Does clarity of responsibility constrain incumbent behavior? As stated above, electoral accountability can be defined in terms of the extent to which voters can use elections to constrain incumbent politicians' behavior so that they act in voters' best interest. In spite of this, the literature on clarity of responsibility has almost exclusively been interested in whether clarity affects voter behavior and paid little attention to incumbent behavior (for a possible exception, see Tavits, 2007). Instead, previous literature has, implicitly, assumed that when voters' mode of selection changes (i.e., when voters become more electorally responsive to the quality of policy outcomes), selection and sanctioning mechanisms automatically kick in, changing incumbent behavior along the way.

This is not an unreasonable assumption. As was discussed above, politicians do seem to respond to changes to their re-election incentives (cf. Alt and Lassen, 2006). Even so, different factors might prevent the effect of voter behavior from translating into changes in incumbent behavior. For one, even if centralization of power increases levels of retrospective voting, the effect might not be big enough to leave a mark on incumbent behavior (for an argument along these lines, see Cheibub and Przeworski, 1999). Another possibility is that the type of retrospective voting behavior that the electorate engages in is too myopic and haphazard for it to change incumbent behavior (for an argument along these lines, see Achen and Bartels, 2016). At present, however, we do not know.

C: Does the length of an incumbent government's time in office affect the extent to which voters hold this government electorally accountable for policy outcomes? Only a small number of studies have dealt with the relationship between time in office and retrospective voting. These studies have generally conceptualized tenure as a component of clarity of responsibility (Nadeau, Niemi and Yoshinaka, 2002), asserting that incumbents' influence on policy outcomes increases with their time in office, and therefore one should expect retrospective voting to be *more* prevalent in elections which feature more experienced incumbents and *less* prevalent in elections which feature less experienced incumbents (Carey and Lebo, 2006; Lebo and Box-Steffensmeier, 2008; Singer and Carlin, 2013). Even so, it might not be rational for voters to let policy outcomes count for more as time in office increases.

To see why this is the case, we need to consult models of Bayesian learning. These models tell us that beliefs which are based on more information are generally less likely to be moved by a single new piece of evidence (e.g., Gerber and Green, 1999). In the context of retrospective voting, one can conceptualize this “information” as all of the relevant cues about the quality of the incumbent obtained before one takes the present state of policy outcomes, the “new piece of evidence”, into account. Information naturally accumulates with time in office. That is, voters will always have more information about their incumbent at $t = x + 1$ than they have at $t = x$, because all of the information accumulated by $t = x$ is also available at $t = x + 1$. Accordingly, as an incumbent’s time in office increases, voters’ stock of relevant information about the incumbent also increases, strengthening voters’ beliefs about the incumbent, leaving these beliefs less malleable, and potentially attenuating the effect the (current) state of policy outcomes have on these beliefs.

Note that Bayesian learning thus suggests that it might be rational for voters who want to select the most competent incumbent to focus *more* on the quality of policy outcomes when the incumbent has *less* power to shape these outcomes. This implication makes time in office an interesting limit case to explore in the context of this dissertation’s research question, because it suggests that some factors which increase governments’ power over policy outcomes might not have the effect on electoral accountability that the clarity of responsibility hypothesis suggests.

D: Can a lack of clarity of responsibility create a self-serving bias in how voters attribute responsibility? As mentioned above, Parker-Stephen (2013) argues in a recent article that high levels of clarity of responsibility for policy outcomes make partisan-motivated reasoning about policy outcomes – an important perceptual bias (cf. Campbell et al., 1960) – more prevalent. This represents one of the only attempts to examine whether clarity of responsibility affects voters’ political cognition. This dissertation continues in this vein, considering a different, and hitherto unexplored, psychological bias.

In particular, drawing on the social psychological concept of a self-serving bias in attribution, it seems reasonable to surmise that if it is hard for voters to find out whether the government or they themselves are responsible for a particular policy outcome, then they will attribute responsibility for this outcome self-servingly. If voters, for instance, face increasing mortgage payments, they will tend to blame the government’s reckless policies; however, if mortgage payments are decreasing, they will tend to credit their own prudent real-estate investment. More generally, one might expect that voters tend to over-emphasize the government’s role in producing

undesirable outcomes, exculpating themselves from any potential blame, and under-emphasize the government's role in producing desirable outcomes, taking personal responsibility instead.

By exploring the self-serving bias in attribution of political responsibility, it is also possible to get at a broader question: namely whether *un*clarity of responsibility simply leads voters to rationally reduce the extent to which they hold incumbents accountable, or whether the ambiguity might sometimes interact with voters' psychological biases, leading them to hold the incumbent *more* accountable than the traditional clarity of responsibility literature would predict. If such a pattern is identified, it might explain why voters sometimes hold incumbent politicians accountable for outcomes they are only tangentially responsible for.⁵

From Four Questions to One Dissertation

These four different questions might seem disparate and a little scattered. This is a result of the fact that this is an article based dissertation, which means that there is a limit to how logically connected the over-arching argument can be, as the individual research articles need to serve their own particular ends. Even so, each of the four questions laid out above follow from the dissertation's research question laid out in chapter 1.

To illustrate how the different questions relate to each other, figure 2.1 draws a stylized path model of how electoral accountability works. In the model, the government affects policy outcomes, which then affect the life of the voters, who then decide whether to support the government. A decision that determines which government is allowed to affect policy outcomes. At the center of the model is the key dependent variable of the dissertation, namely, governments' power over policy outcomes. Three arrows go from this variable to different parts of the accountability process. Next to the arrows are letters signifying which question looks at the relationship between governments' power over policy outcomes and this particular part of the accountability process. A brief justification for the placement of each question in the model follows below:

1. Question A and C both deal with how governments' power over policy outcomes condition voters decision to either support or reject the incumbent government on election day, and are therefore placed next to the arrow in the bottom right corner.

⁵For a whimsical example of this, see: <http://www.youtube.com/watch?v=RDocnbkHjhI&t=1m0s>.

2. Question B deals with how governments' power over policy outcomes conditions which policy outcomes the governments pursue, and is therefore placed next to the arrow in the top left corner.
3. Question D deals with how limitations to governments' power over policy outcomes create a self-serving bias in how the voters interpret policy outcomes, and is therefore placed in the bottom left corner.

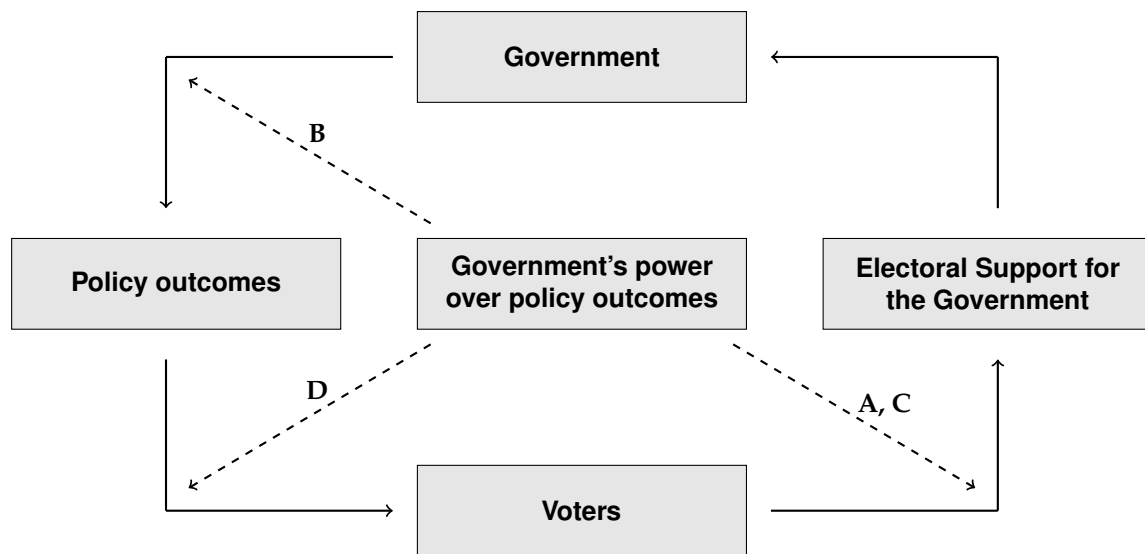


Figure 2.1 How the different contributions of the dissertation fit into a stylized electoral accountability model. Letters refer to the different research articles: A 'Responsibility and Accountability', B 'Incumbent Behavior', C 'Incumbent Tenure', and D 'Self-servingness'.

The model is, of course, an abstraction. In reality, each question also touches upon different aspects of the accountability process than the one displayed in figure 2.1. This will become apparent once the dissertation gets to the individual research articles which try to answer these questions. For instance, the article which deals with question A also examines whether governments' power over policy outcomes changes voters perceptions of policy outcomes, and the article which deals with question B also looks at how voters mode of selection changes. Even so, the placement in the stylized model does correspond to what process the core question in each article revolves around.

Chapter 3

Research Designs

The dissertation faces two major inferential problems when it comes to exploring how the extent of governments' power over policy outcomes shapes patterns of electoral accountability. First, governments' abilities to influence policy outcomes is not randomly assigned across contexts. There are reasons why some political systems grant extensive power over policy outcomes to the government, while other political systems impose limits on executive discretion. This presents a serious challenge to causal identification, because one might mistake the effect of factors that make power-sharing institutions more likely for the effect of power sharing. Second, in some instances, the dissertation wants to identify the effect of governments' power over policy outcomes on the extent to which voters hold incumbent governments accountable for the quality of policy outcomes. This presents a separate type of causal inference problem, as we need to estimate the effect of one variable (government power) on the effect of another variable (quality of policy outcomes). This chapter lays out these inferential problems and explains how the research designs employed in the different research articles address these problems.

The Key Inferential Problem: Causal Identification

The goal of this dissertation is to explore how the extent of governments' power over policy outcomes shapes patterns of electoral accountability. In particular, as laid out in chapter 2, the dissertation wants to look at how the centralization of power over policy outcomes in the hand of the executive affects the extent to which voters hold this executive accountable for specific outcomes (articles A and C), how it affects the policy outcomes pursued by the incumbent (article B) and how it affects the attributions made by the voter (article D). How can one make inferences about the effect of governments' power over policy outcomes on these variables?

Let us say that we want to know what happens to some variable, R , if a government's power over policy outcomes, GP , increases. For simplicity, let us restrict the government's power to have two values, $GP \in \{0, 1\}$, and say that both R and GP are measured for a specific incumbent government i . As a running example, let R_i be the tax policy enacted by some incumbent i , and let GP be an indicator of whether the incumbent can set tax policy alone ($GP_i = 1$) or whether the incumbent's tax policy has to be approved by another party before it is enacted ($GP_i = 0$).

Using the potential outcomes framework (Angrist and Pischke, 2008; Holland, 1986), we can imagine R_i in two states. One is R_{1i} , which is the level of R for incumbent i when $GP = 1$, and the other is R_{0i} , which is the level of R for incumbent i when $GP = 0$. We are interested in the difference between the two states, $R_{1i} - R_{0i}$, which represents the causal effect of getting more power over policy outcomes on R for incumbent i . Unfortunately, we cannot observe $R_{1i} - R_{0i}$ directly, because R_{1i} is only realized for the subset of incumbents where $GP = 1$, and R_{0i} is only realized for the subset of incumbents where $GP = 0$. As such, we do not observe the counter-factual outcomes.

In terms of our example, we observe the tax policy enacted by incumbents who can set tax policy alone (i.e., $R_{1i}|GP = 1$), but we do not observe the tax policy that the same incumbents would have enacted if they had to set tax policy with another party ($R_{0i}|GP = 1$). Similarly, we observe the tax policy enacted by incumbents who have to set tax policy with another party ($R_{0i}|GP = 0$), but we do not observe the tax policy these incumbents would have enacted if they could set tax policy alone ($R_{1i}|GP = 0$).

This is a problem, because this dissertation is, at a fundamental level, interested in such counter-factual outcomes. We want to know what would happen to patterns of accountability if the government had more (or less) power over policy outcomes.

Since the counter-factual outcomes are not observed, we are forced to compare the average of R_i for incumbents where $GP = 1$ to the average R_i for incumbents where $GP = 0$. Following Angrist and Pischke (2008, 14), it is possible to relate this average difference to the average causal effect of GP on R , which is the aggregate level effect we are interested. This can be done by adding in and subtracting the average level of R that incumbents with more power over policy outcomes would have had if they had less power over policy outcomes ($E(R_{0i}|GP_i = 1)$). This gives us

$$E(R_{1i}|GP_i = 1) - E(R_{0i}|GP_i = 0) = E(R_{1i}|GP_i = 1) - E(R_{0i}|GP_i = 1) + E(R_{0i}|GP_i = 1) - E(R_{0i}|GP_i = 0) \quad (3.1)$$

The right hand side of this equation consists of two average differences. The first average difference is the average effect of GP on R among the subset of incumbents for whom $GP = 1$ (in the language of experimental research this corresponds to the average treatment effect of the treated).¹ The second is the average difference in R between the incumbents for whom $GP = 1$ and the incumbents for whom $GP = 0$, which would have existed if $GP = 0$ for all incumbents. Angrist and Pischke (2008) label this term the selection bias, because it captures the extent to which incumbents with specific levels of R select into (or are selected into) specific levels of GP independent of any effect R might have on GP . So, how does this relate to the running example?

If we look at the average difference in tax policy between incumbents who can set tax policy alone and those who have to set tax policy with another party, then equation 3.1 tells us that this difference will reflect (1) the average effect that the ability to set taxes independently has on tax setting, and (2) the average difference in tax policy between those who set tax policy alone and those who do not, which would have existed *even if* all incumbents had to set tax policy with another party.

This discussion reveals the key inferential problem this dissertation faces: when comparing governments who have more power over policy outcomes with those who have less, one risks confounding the effect of power over policy outcomes with differences which would have been there in the absence of any disparity in power over policy outcomes.

¹While not the effect for the individual incumbent i , getting at this average causal effect gets us closer to answering our research question.

At the same time, the discussion also reveals how this inferential problem can be solved. In particular, if it is possible to argue that the following inequality holds

$$E(R_{0i}|GP_i = 1) = E(R_{0i}|GP_i = 0), \quad (3.2)$$

then the average observed difference will reflect the average causal effect of governments' power over policy outcomes.²

This equality states that if more power over policy outcomes had not been assigned to incumbents, then there would have been no difference, on average, in R between incumbents assigned more and less power over policy outcomes. Accordingly, equality 3.2 will only hold if the assignment to more power over policy outcomes is independent of R : that is, if the factors that make the assignment of more power over policy outcomes more likely are unrelated to levels of R .

Equality 3.2 will probably not hold in most cases, because forces that affect the extent of governments' power over policy outcomes will tend to have an independent effect on relevant R s as well. Box 3.1 argues this point using our running example. This does not mean, however, that equality 3.2 never holds; it just means that when examining the effect of governments' power over policy outcomes, one needs to devise an empirical strategy that makes it possible to plausibly claim that this equality holds, so that causal identification is possible. Below, it is detailed how the dissertation uses randomized vignette survey experiments and quasi-experiments set in Danish municipalities to devise such identification strategies.

Using Randomized Survey Experiments

In article D, 'Self-serving Bias', the dependent variable is voters' attributions about various policy outcomes. The independent variable is the nature of the policy outcome; in particular, whether the relative role played by the government and the voter in producing this outcome is ambiguous, and whether the outcome is positive or negative. Using a survey experimental design, a population based sample of Danish voters are presented with different hypothetical outcomes, and then asked to evaluate how responsible the government would be if these hypothetical outcomes were realized. In order to make sure that equality 3.2 holds, the different hypothetical outcomes are randomly assigned to participants. Using random assignment ensures that the participants assigned to different outcomes are – in expectation – equal on

²If the effect size depends on assignment to 'treatment' ($GP = 1$), then the average difference will only reflect the causal effect of GP on R among those for whom $GP = 1$.

Box 3.1: Illustrating why equality 3.2 rarely holds

Say we wanted to look at whether the scope of executive discretion with respect to fiscal policy (GP) has an impact on tax policy (R). To that end, we compare tax levels across the following indicators of executive discretion: economic openness (more open ctr. more closed), type of government (coalition ctr. single party), and federalism (federalist ctr. unitary). Would differences in tax policy across these different indicators reflect the causal effect of executive discretion? To answer this question, we need to evaluate whether equality 3.2 holds.

- Levels of economic openness: Equality 3.2 probably does not hold, because countries that select into more economic openness are probably also more economically libertarian, leaving tax levels uneven across levels of economic openness even in the absence of any independent effect of openness on tax setting.
- Coalition ctr. single party governments: Equality 3.2 probably does not hold because opposition parties that select into coalition governments might only do so if they expect some public spending to come their way, driving up public spending and, in turn, taxes. This makes tax levels uneven across types of governments even in the absence of any independent effect of government type on tax setting.
- Federalist ctr. unitary political systems: Equality 3.2 probably does not hold because countries that select into federalism might do so because of strong regional political identities, which could curtail public spending at the national level. This will leave tax levels uneven across types of political system even in the absence of any independent effect of federalism on tax setting.

all observed and unobserved characteristics, including how inclined the participants would be to hold the government responsible for policy outcomes.

While the controlled setting of the survey experiment allows for a simple and strong identification strategy, the control comes at a cost. For one, people cannot be exposed to actual policy outcomes in the context of a survey, forcing us to use hypothetical outcomes. In addition to this, people are asked to evaluate outcomes in a serene survey setting, rather than in a real-life setting, where their attributions might be affected by conversations with friends and family as well as messaging from political elites. These abstractions probably distort the type of conclusions that can be drawn from the survey experiment (cf. Barabas and Jerit, 2010); however, it is hard to know how much and in what way they do this. In order to ameliorate these concerns, article D supplements the survey experiment with several observational studies, which look at associations between the policy outcomes experienced by voters and the attributions voters make. By using this data, the article explores whether it is possible to recognize the well-identified effect from the survey experiment in a setting where voters evaluate actual policy outcomes.

Using Danish Municipalities as Laboratories

One way to meet the identifying assumption laid out in equation 3.2, while retaining some measure of ecological validity, is to look for quasi-experiments: instances where nature assigns governments' power over policy outcomes in a way that is independent of factors that affect the dependent variables we are interested in (Dunning, 2012). This is the main approach taken in articles A, B and C, which examine the effect of governments' power over policy outcomes on levels of retrospective voting (A, C) and incumbent behavior (B).

The quasi-experiments examined in the dissertation have a common backdrop: they examine subnational variation, in the extent to which local governments in Denmark have power over local policy outcomes. Interestingly, while subnational variation has often been leveraged to study electoral accountability (e.g., Alt and Lowry, 1994; Alt, De Mesquita and Rose, 2011; De Janvry, Finan and Sadoulet, 2012; Mouritzen, 1989; Sances, 2016, 2017), it has rarely been used in the context of studying how governments' power over policy outcomes affects patterns of electoral accountability. This is somewhat surprising, as many of the potential confounders that threaten causal identification are at the country level (cf. box 3.1). This is partly a reflection of the fact that there is no force constraining between country differences

in political institutions, whereas considerations related to equal protection typically limit the institutional differences between subnational units. Further, national level institutions rarely change dramatically, in part because of constitutional constraints. In contrast, local governing structures are often tweaked and altered at the whims of central governments. This make local governments fertile ground for identifying potential quasi-experiments.

Since Danish municipalities provide the empirical context for three of the research articles in the dissertation, box 3.2 provides some context on their function, history and political system.

Three Quasi-experiments

Having discussed their shared empirical backdrop, I now turn to laying out the details of the specific quasi-experiments.

The quasi-experiment used in article B, 'Incumbent Behavior', identifies the effect of incumbents' power over policy outcomes on a specific type of incumbent behavior (tax setting) by comparing city councils where the largest party narrowly won a majority of the seats, giving them the opportunity to select the mayor and the chairmen of all committees unilaterally, with city councils where the largest party narrowly lost the majority, forcing them to collaborate with other parties. This type of quasi-experiment is called a regression discontinuity design (RDD), because it relies on the fact that there is a discontinuity in some continuous variable (i.e., proportion of votes) where the treatment (more power over policy outcomes) is assigned (cf. Dunning, 2012; Lee, 2008). The identifying assumption here is that equality 3.2 holds when the assignment to single party majority status is based on a small number of votes. This is a reasonable assumption to make since the exact vote totals parties receive at elections are influenced by as-if random forces that are unrelated to levels of electoral accountability, such as the weather or traffic patterns (for evidence of this, see Eggers et al., 2015).

The quasi-experiment used in article A, 'Responsibility and Accountability', takes place in the wake of a 2007 reform. In particular, it looks at the effect of a labor market reform that gave 14 (out of 98) Danish municipalities more power over a specific policy outcome: unemployment services.³ The article identifies the effect of the

³In Denmark, active labor market policies have played an important role in efforts towards reducing structural unemployment (Torfing, 1999). This reform gave the selected municipalities the sole responsibility for managing the provision of these services, a responsibility which was shared before the reform. Responsibility for unemployment services was eventually transferred to all municipalities.

Box 3.2: Some Context About The Danish Municipalities**Municipal tasks**

- Denmark is a decentralized welfare state where municipalities can affect their local revenue and set a yearly budget.
- Municipal tasks and services include the core welfare services of the Danish welfare state and municipal spending amounts to 35 percent of GDP, which is more than half of all public spending. The tasks include primary education, child care and care for elderly people, libraries, local sports facilities and other cultural activities, granting and payment of cash assistance, anticipatory pension and certain other social benefits, job activation and employment projects for unemployed persons (unemployment services), public utilities, environmental measures and emergency services.

Political System

- Danish municipalities are governed by local councils which are elected at proportional elections and with a multi-party system which, to a large extent, mirrors the party system at the national level (Blom-Hansen, Elklit and Kjær, 2017).
- Elections are fixed to take place every four years and do not usually coincide with elections at the national or EU level. Turnout is high with an average of around 70 percent since 1970.
- The work in the local council is structured by a number of committees. The number and size of the committees is determined by the council. Committee membership is allocated proportionality between the political parties which means that there is broad political representation in all committees. The committees can decide on matters in their area and the administrative responsibility across areas is essentially divided.
- Following each municipal election, a majority in city council elects a mayor, and the chairmen of the various committees (Serritzlew, Skjæveland and Blom-Hansen, 2008). Mayors are the only full time professional politicians in the city councils and have a number of formal obligations (Kjaer, 2015). Mayor are also responsible for the day-to-day business of the administration and chairs the important economic committee which sets taxes and the budget.

Organization and History

- There have been two large municipal reforms in the last 50 years.
- The first was conducted in 1970 as the Danish welfare state started to expand. Here the number of municipalities were reduced from more than 1000 to 275. (Although it was 277 the first two years.) so that the municipalities were able to efficiently provide a more complex portfolio of public services (Ingvartsen, 1991).
- The second reform was conducted in 2007 and further reduced the number of municipalities from 275 to 98. Once again, the increasing complexity of public service provision was a key argument for the reform (Christiansen and Klitgaard, 2008).

increased power over policy outcomes on electoral accountability by comparing the extent to which the mayoral party was held electorally accountable for the quality of unemployment services in the 14 ‘treatment municipalities’ and in the 84 ‘control municipalities’. It is not obvious that this comparison satisfies equality 3.2; however, the article argues that it does, because the national government, which assigned municipalities to treatment and control, were interested in designing two very similar groups for political reasons. The article backs up this claim by showing that: (1) there were no differences between treatment and control municipalities on observables that might be correlated with electoral accountability before implementation of the reform, (2) there were no difference in how electorally accountable mayoral parties were held for policy outcomes unrelated to unemployment services, and (3) there were no apparent differences across treatment and control municipalities in the extent to which mayoral parties were held electorally accountable for unemployment services before the reform.

The quasi-experiment used in article C, ‘Incumbent Tenure’, also takes place in the wake of the municipal reform in 2007. In particular, it exploits the fact that, as a result of the reform, some municipalities were amalgamated. In some of the recently amalgamated municipalities’ parts of the electorate had a long history with the incumbent mayoral party, whereas others, as a result of the amalgamation process, had to evaluate the incumbent mayoral party for the first time. Comparing the extent to which incumbents were held electorally accountable for recent local changes in the unemployment rate by ‘new’ ctr. ‘old’ voters, the article tries to identify the effect that the length of voters’ prior experience with an incumbent has on the extent to which they hold these incumbents accountable for the current state of policy outcomes.

Two things are of note here. First, the article does not look directly at the effect of the individual local government’s power over policy outcomes, but, instead, at the effect of the length of voters’ prior experiences with the incumbent. This logic behind this is explored further in the article; however, the logic behind this inferential strategy is as follows: if we can show that the length of voters’ experience with an incumbent has an effect on electoral accountability, then it follows from this that incumbent tenure, which is perfectly collinear with the length of voters’ experience with an incumbent, also affects electoral accountability. Second, while the amalgamations were exogenous, in the sense of being unrelated to the voting decision of individual voters, the amalgamation process did not happen randomly or as-if randomly, raising

the possibility that equation 3.2 does not hold.⁴ To address both the fact that we do not look directly at differences in governments' power over policy outcomes, and the fact that the identifying assumption might not hold, the article includes two additional country-level studies. Both compare the extent to which the *same* incumbent is punished for economic conditions early contra late in their term.

A Note on Generalizability

One might contend that relying so heavily on quasi-experiments from a single setting severely delimits the dissertation's scope of inference. This is true in the sense that it would be better to have quasi-experiments that covered other political systems as well, a point I return to when discussing the overall limitations of the dissertation in chapter 5. However, it is not clear that the dissertation would have been well served by focusing less on these quasi-experiments and more on cross-country comparisons.

For one, as should be clear from the discussion in chapter 2, the clarity of responsibility literature, which has been the main vehicle for studying how governments' power over policy outcomes shapes electoral accountability, has seen its share of cross-country studies. Accordingly, in the context of the larger research program, the marginal returns from another such study is probably limited. By contrast, the type of study conducted in this dissertation, which looks at more local, well-identified effects is less common in the literature.

Further, while conventional wisdom tells us that if we focus on a more diverse set of cases (e.g., a dataset which includes a large number of different countries), then the prospects for generalizability increase, a recent article by Samii (2016) argues that this is in fact not the case.

"This type of generalizing reflects a presumption that by using a data set representative of some target population [...] the study will produce findings generalizable to that population. [...] Statistically speaking, this line of reasoning is completely misguided. [...] There is no clear ordering of experiments, quasi-experiments, and observational studies that use regression or other control methods in terms of the generality of their findings." – Samii (2016, 943-945)

⁴It is unclear to what extent this is a problem. Bhatti and Hansen (2011) find that political and economic homogeneity, factors which are probably related to electoral accountability, did *not* play a large role with respect to which municipalities were amalgamated.

Samii reaches this conclusion by arguing that when standard observational studies use some form of statistical control (e.g., control variables) they also limit the kind of variation in the independent variable which is used to identify the effect on the dependent variable, and, as a result, the estimated effect will often be just as 'local' (i.e., derived from a similarly limited set of cases) as it is in experimental or quasi-experimental studies. If one, for instance, examines the effect of government type (single ctr. multi-party government) on the extent of retrospective voting using a cross-country regression model with a large battery of controls, including year and country fixed effects, then the identified effect of government type will only be ascribable to the variation in government type that is independent of these controls. Say that government type only varies very little in one country (e.g., the UK) and in other countries it is very closely related to other controls (e.g., to the effective number of parties), then observations from these countries will not contribute to the effect estimate and, as a result, the effect cannot be generalized to these countries (see also Aronow and Samii, 2016).

In summary, it is not clear that the dissertation loses more generality by looking at Danish municipalities than it would have if it did a well-controlled cross-country study, and it is pretty clear that the marginal returns, in terms of new insights, from looking at Danish municipalities will be higher than if the dissertation contributed to the existing literature by employing yet another set of cross-country studies.

Measuring Retrospective Voting

Research articles A 'Responsibility and Accountability' and C 'Incumbent Tenure' both examine the effect of governments' power over policy outcomes on the extent of retrospective voting. That is, whether voters hold their government more or less accountable for policy outcomes when their government's power over these policy outcomes increase. This adds a layer of complexity, because measuring the level of retrospective voting is tantamount to identifying a causal effect; namely, the casual effect of policy outcomes on support for the incumbent. What implications does this have for our analyses?

To explore this, let us first define the retrospective voting a group of incumbents face as the average difference in electoral support (Y) these incumbents would face if policy outcomes (PO) were of a high quality ($E(Y_{1i})$) as opposed to a low quality ($E(Y_{0i})$). When measuring retrospective voting, we cannot get at this counter-factual directly, forcing us to look at the average difference in electoral support between

incumbents who oversee high-quality policy outcomes ($E(Y_{1i}|PO = 1)$) and incumbents who oversee low-quality policy outcomes ($E(Y_{0i}|PO = 0)$). Applying the potential outcomes framework, this average difference can be described as

$$E(Y_{1i}|PO_i = 1) - E(Y_{0i}|PO_i = 0) = E(Y_{1i}|PO_i = 1) - E(Y_{0i}|PO_i = 1) + E(Y_{0i}|PO_i = 1) - E(Y_{0i}|PO_i = 0), \quad (3.3)$$

which reflects actual retrospective voting (i.e., the effect of policy outcomes on support for the incumbent), as well as some 'selection bias' (the extent to which incumbents with higher levels of electoral support select into high-quality policy outcomes).

This dissertation, however, is not interested in measuring the level of retrospective voting. Rather, it is interested in measuring the effect governments' power over policy outcomes (GP) has on retrospective voting (i.e., an interaction effect). This complicates the analysis, because we now have four potential states of the world ($PO \in \{0, 1\} \times GP \in \{0, 1\}$), only one of which is realized. To describe these different states, we define the electoral support of an incumbent i as Y_{jti} , where the subscript j relates to whether the incumbent is in a state where government's power over policy outcomes is high (Y_{1ti}) or in a state where it is low (Y_{0ti}), and the subscript t denotes whether the incumbent is in a state where policy outcomes are high quality (Y_{j1i}) or in a state where they are low quality (Y_{j0i}).

For each individual incumbent i , we are interested in whether the quality of policy outcomes has a larger effect when the government's power over policy outcomes is large as opposed to when it is small. In effect, we are thus interested in the following difference-in-difference: $Y_{11i} - Y_{10i} - (Y_{01i} - Y_{00i})$.

Since only one outcome for each i is observed, this difference-in-difference term will include several counter-factual components for each incumbent, and we are therefore forced to look at the observed average levels of electoral support across incumbents with different levels of power over policy outcomes, and who oversee policy outcomes of varying quality. In particular, we can estimate the following average difference-in-difference without relying on any counter-factuals

$$E(Y_i|GP_i = 1, PO_i = 1) - E(Y_i|GP_i = 1, PO_i = 0) - (E(Y_i|GP_i = 0, PO_i = 1) - E(Y_i|GP_i = 0, PO_i = 0)). \quad (3.4)$$

To find out what relationship this estimate has to the average causal interaction effect, we can add in and subtract the relevant counter-factual outcomes.⁵ If we do this and assume that equality 3.2 holds across levels of PO ,⁶ we find that

$$\begin{aligned}
 & E(Y_i|GP_i = 1, PO_i = 1) - E(Y_i|GP_i = 1, PO_i = 0) - \\
 & (E(Y_i|GP_i = 0, PO_i = 1) - E(Y_i|GP_i = 0, PO_i = 0)) = \\
 & E(Y_{11i}|GP_i = 1, PO_i = 1) - E(Y_{10i}|GP_i = 1, PO_i = 1) - \\
 & (E(Y_{01i}|GP_i = 1, PO_i = 1) - E(Y_{00i}|GP_i = 1, PO_i = 1)) \\
 & \underbrace{\hspace{15em}}_{\text{Average effect of } GP \text{ on effect of } PO \text{ for } i \text{ s with } GP = 1 \text{ \& } PO = 1} \quad (3.5) \\
 & + E(Y_{10i}|GP_i = 1, PO_i = 1) - E(Y_{10i}|GP_i = 1, PO_i = 0) - \\
 & (E(Y_{00i}|GP_i = 0, PO_i = 1) - E(Y_{00i}|GP_i = 0, PO_i = 0)). \\
 & \underbrace{\hspace{15em}}_{\text{Difference in selection bias into } Y \text{ across } PO}
 \end{aligned}$$

This equation tells us that if we look at the observed differences in the level of retrospective voting incumbents face across levels of government power over policy outcomes, then we end up with the causal interaction effect (the first two right hand side lines), as well as a “difference in selection bias” term (the final two lines).

The “difference in selection bias” subtracts (1) the extent to which powerful incumbents ($GP = 1$) with higher levels of electoral support select into high quality policy outcomes; from (2) the extent to which less powerful incumbents ($GP = 0$) with higher levels of electoral support select into high quality policy outcomes. If (1) and (2) are equal, it means that the difference in electoral support across incumbents assigned to high – as opposed to low – quality policy outcomes, which would have existed if all incumbents were assigned to low-quality policy outcomes, is the same for more and less powerful incumbents.

⁵In particular, we add in and subtract $-E(Y_{10i}|GP_i = 1, PO_i = 1) - (E(Y_{01i}|GP_i = 1, PO_i = 1) - E(Y_{00i}|GP_i = 1, PO_i = 1))$ and $-E(Y_{00i}|GP_i = 0, PO_i = 1) - (E(Y_{00i}|GP_i = 1, PO_i = 0) - E(Y_{00i}|GP_i = 0, PO_i = 0))$.

⁶We assume that $E(Y_{01i}|GP_i = 1, PO_i = 1) = E(Y_{01i}|GP_i = 0, PO_i = 1)$ and $E(Y_{00i}|GP_i = 1, PO_i = 0) = E(Y_{00i}|GP_i = 0, PO_i = 0)$. Substantively, this assumption means that assignment to $GP = 1$ is unrelated to the average electoral support that incumbents would have gotten if all incumbents had been assigned to $GP = 0$ both when policy outcomes are high quality ($PO = 1$) and when policy outcomes are low quality ($PO = 0$).

Importantly, if (1) and (2) are equal, they cancel each other out, leaving us with an unbiased estimate of the average effect of governments' power over policy outcomes on the effect of policy outcomes on incumbent support (i.e., the causal interaction effect). This is the additional identifying assumption that one needs to make in order to identify the causal effect of governments' power over policy outcomes on retrospective voting. The assumption can be expressed formally as

$$\begin{aligned} E(Y_{10i}|GP_i = 1, PO_i = 1) - E(Y_{10i}|GP_i = 1, PO_i = 0) = \\ E(Y_{00i}|GP_i = 1, PO_i = 1) - E(Y_{00i}|GP_i = 1, PO_i = 0). \end{aligned} \quad (3.6)$$

This equality can hold even in contexts where there is some selection by incumbents into policy outcomes of a particular quality. As long as the selection pattern is the same for incumbents with more and less power over policy outcomes, it does not bias the estimate of the causal interaction effect.

It will, therefore, sometimes be possible to estimate an unbiased effect of governments' power over policy outcomes on retrospective voting in contexts where one would normally overestimate or underestimate the level of retrospective voting. In particular, if the amount of over- or underestimation is constant across different levels of governments' power over policy outcomes, then causal identification is possible. How can this be the case? One can think of the issue as one of measurement error in the dependent variable: as long as it is uncorrelated with the independent variable, it will generally not bias the causal estimate (King, Keohane and Verba, 1994, chap. 5).⁷ Similarly, even if the level of retrospective voting is estimated with some error, it will not bias the causal estimate if this error is uncorrelated with governments' power over policy outcomes.

When might equality 3.6 not hold? Increasing an incumbent's power over a set of policy outcomes might increase the extent to which voters rely on their partisan predispositions when evaluating these policy outcomes (for evidence of such a mechanism, see the study by Parker-Stephen, 2013, which was discussed at length in chapter 2). That is, voters who like the incumbent might select into positive perceptions of policy outcomes, whereas voters who do not might select into negative perceptions, and they might do so to a greater extent when incumbents have more

⁷The issue can also be compared to the parallel trends assumption in difference-in-difference models, where base-line differences in the dependent variables across the treatment variable may differ for reasons that are unrelated to the treatment, as long as the trends in the dependent variable are independent of factors that affect the likelihood of being assigned to treatment (Angrist and Pischke, 2008, 230).

power over policy outcomes. This means that voters' evaluations of the quality of policy outcomes will reflect whether voters support the incumbent (i.e., selection bias), and that this reflection will be stronger when governments' power over policy outcomes is more extensive (i.e., difference in selection bias). If this is the case, one will tend to over-estimate the level of retrospective voting more when incumbents have more power over policy outcomes, and equality 3.6 will therefore not hold.

Articles A and C, which use levels of retrospective voting as the dependent variable, are sensitive to these issues and try to examine whether equation 3.6 does – in fact – hold. For instance, in article A, I look at whether there are any signs that voters' evaluation of policy outcomes depend more on their attachment to the incumbent party when the incumbent party has more control over policy outcomes (see section S8 of the supplementary materials in chapter 6 for details). If this is the case, equality 3.6 will not hold, because the level of retrospective voting will be over-estimated *more* when governments' power over policy outcomes is higher. In article C, I address similar concerns by using an instrumental variables approach. In particular, I instrument voters' evaluation of a policy outcome using a set of objective indicators of this policy outcome's quality, thereby mitigating any differences in the strength of voters' partisan "perceptual screens" (see section S13 of the supplementary materials in chapter 8 for details).

In summary, if equalities 3.2 and 3.6 hold, then observed differences in the level of retrospective voting faced by governments with different levels of power over policy outcomes will reflect the causal effect of governments' power over policy outcomes on the effect that the quality of these policy outcomes have on support for the incumbent. We are sensitive to this set of more extensive identifying assumptions in articles A and C, which examine the effect of governments' power over policy outcomes on the extent of retrospective voting.

An Overview of Data Sources and Research Designs

Table 3.1 summarizes this chapter by providing an overview of the data sources and identification strategies employed in the individual research articles.

Table 3.1 Overview of data sources and research designs employed in the individual research articles

Research Question	Data Sources	Identification Strategy
A: What characterizes the relationship between a government's power over a specific policy outcome and how electorally accountable they are held for the quality of this outcome?	The Danish Municipal Election Survey (2009, n=1,522, responses cluster sampled at the municipal level). Also includes Danish Municipal Election Surveys from 2005 (n=2,011) and 2013 (n=3,986) to estimate a difference-in-difference model and make additional robustness checks.	Quasi-experiment where assignment to early implementation of a labor market reform manipulated mayors' power over unemployment services in a subset of the Danish municipalities.
B: Does the extent of incumbents' power over policy outcomes affect their propensity to pursue the type of policy outcomes that the electorate prefers?	A strongly balanced panel of municipal elections and local income tax rates (1972–2005, n=2,820).	Comparing city councils where the largest party narrowly won or lost a majority of the seats in the city council (Close election RDD).
C: Does an incumbent's time in office affect the extent to which they are held electorally accountable for the current state of policy outcomes?	A dataset of elections from 41 different countries (n=409). A collection of sixty election surveys from the European Election Studies. The dataset covers 10 countries across 25 years (n=39,556). Precinct level election returns from the 2009 Danish municipal elections (n=1,465).	Comparisons of the same incumbent across different levels of tenure (survey and country-level data). Adjustment for selection on observables using fixed-effects regression. Within municipality comparisons of voters with different levels of experience with the same mayoral party (subnational data).
D: Is there a self-serving bias in the way voters attribute political responsibility for the quality of policy outcomes?	Survey experiments conducted in Denmark. Respondents asked to evaluate how responsible the government was for randomly assigned hypothetical economic outcomes (n=1,002, n=263). Survey conducted in Denmark on attributions and perceptions of the economy (n=933). The American National Election study (n=13,306), the Danish National Election Study (n=13,293) and the Latinobarometro (n=143,191).	Random assignment of hypothetical economic events to voters (survey experiment). Comparison of voters with different economic experiences (survey data).

The sample sizes referred to above is the effective sample size (i.e., number of observations excluding missing data).

Chapter 4

Key Findings

This chapter gives an overview of the central findings from the four different research articles. The chapter begins by discussing how governments' power over policy outcomes affects voter behavior. Then it turns to how governments' power over policy outcomes shapes the behavior of incumbent politicians. Finally, the chapter discusses how voters' attributions are affected by a specific type of constraint on the governments' power over policy outcomes. Note that the analyses presented in this section have been simplified to make for a short and clear presentation. For more elaborate and detailed analyses, as well as additional robustness checks, the reader should consult the individual research articles (chapters six through nine).¹

¹As this chapter serves as a summary of the most important results in the different articles, parts of what follows are taken more or less directly from the different research articles.

Voter Behavior

As discussed at length in chapter 2, the literature on clarity of responsibility has shown that in contexts where governments' power over policy outcomes is more extensive, voters tend to hold the government more electorally accountable for the quality of important policy outcomes. The dissertation engages with this finding in two ways. First, it re-examines the relationship between electoral accountability and governments' power over policy outcomes in a setting where governments' power over a specific policy outcome changed abruptly and exogenously. Second, it examines incumbent tenure, a variable that is related to how much power governments have over policy outcomes, exploring whether this variable moderates voter behavior in the way the clarity of responsibility hypothesis would predict.

Governments' power over policy outcomes has a causal, adaptive and policy-specific effect on the extent of electoral accountability

Will voters hold an incumbent government more electorally accountable for the quality of a policy outcome when the incumbent's control of the underlying policy increases? Article A, 'Responsibility and Accountability', tries to answer this question by studying the electoral consequences of a labor market reform in Denmark that gave 14 municipal mayors more power over unemployment services (i.e., active labor market policy).

This case has three appealing features when it comes to furthering our understanding of the relationship between governments' power over policy outcomes and electoral accountability. First, the reform only affected the municipalities' power over a *single* policy outcome, making it possible to find out whether the relationship between governments' power over policy outcomes and electoral accountability is policy-specific or whether it resides at a more general level. Second, the reform was implemented abruptly, from one election to another, giving us a chance to see whether voters adapt quickly to changes in power over policy outcomes. Third, the national government, which decided what municipal mayors were assigned more power over unemployment services, had no incentive to select municipalities based on extant or potential levels of electoral accountability. In fact, they were under political pressure to pick a set of municipalities that were balanced on observables (i.e., geography and size). As such, it is reasonable to assume that the article can use this reform to estimate the average *causal* effect of power over unemployment services

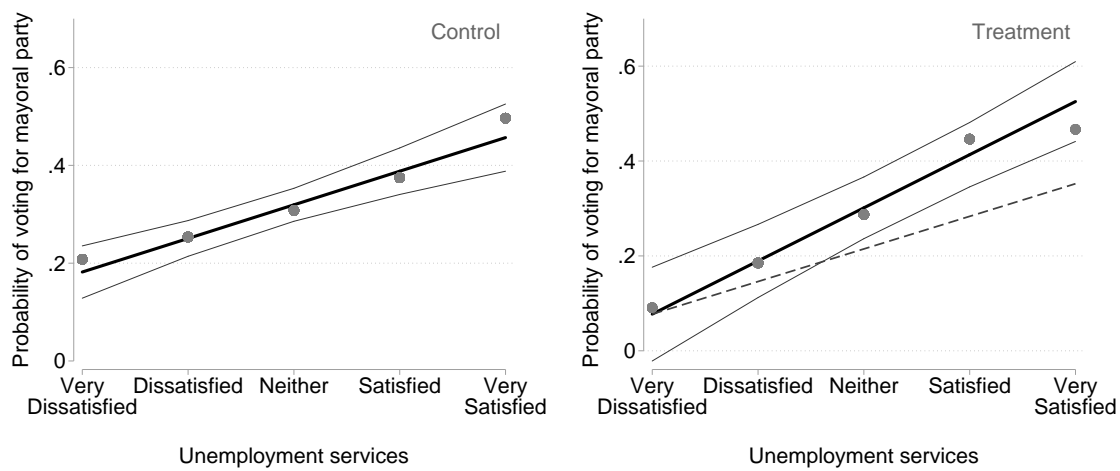
on electoral accountability for these and other services. Additional quantitative and qualitative evidence for this key assertion is provided in the article (chapter 6).

To study the effect this reform had on voter behavior, the article utilizes the Danish Municipal Election survey from 2009 (Elklit and Kjær, 2013), examining the extent to which the incumbent mayoral party was held electorally accountable for unemployment services in the 14 ‘treatment municipalities’ that got more power over unemployment services, and in the 84 ‘control municipalities’ that did not.

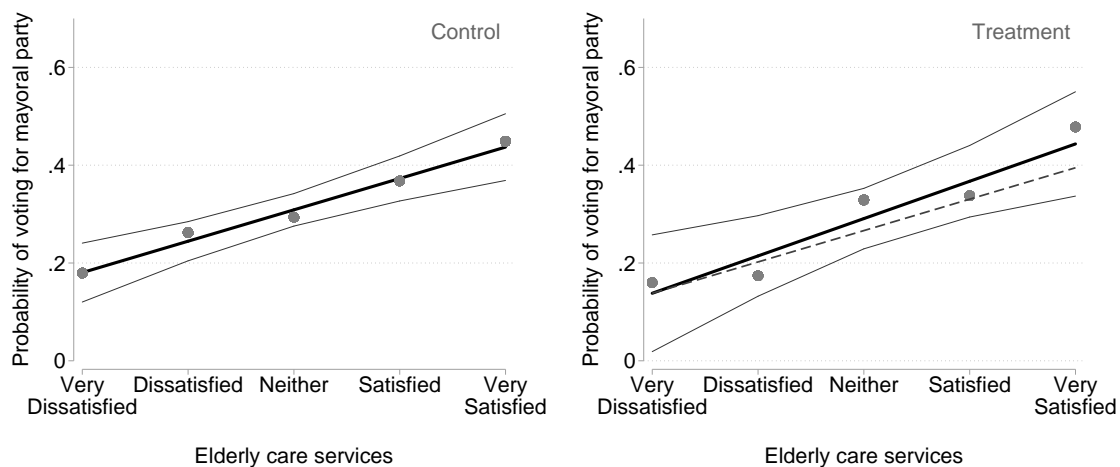
Figure 4.1 presents the main results from article A. As panel (a) of the figure shows, electoral support for the mayor was more closely related to voters’ perception of the quality of unemployment services in the treatment municipalities. Those who thought unemployment services were doing poorly were less likely to support the mayor in the treatment municipalities, and those who thought unemployment services were doing well were more likely to support the mayor in the treatment municipalities. Accordingly, the increase in the local incumbent’s power over unemployment services seems to be associated with an increase in the extent to which voters punished and rewarded the local incumbent for the quality of these services.

Panel (b) of figure 4.1 shows that while there was a difference across treatment and control municipalities with respect to how electorally accountable the mayor was held by voters for unemployment services, there was no corresponding difference with respect to another policy outcome (i.e., the quality of elderly care services). Substantively, this tells us that electorates respond to policy-specific shocks to governments’ power over policy outcomes by adjusting accountability in a policy-specific way. That is, if incumbents get more power over a specific policy outcome, then voters hold the incumbent more electorally accountable for this outcome; but they do not hold the incumbent more accountable for other policy outcomes as well.

Overall, the evidence from article A implies that the electorate moderates the extent to which local governments are held electorally accountable for specific outcomes based on the extent to which local governments had a hand in crafting and implementing the policies that shape this outcome. More broadly, the article implies that if an incumbent is clearly responsible for a clearly demarcated outcome, then voters will hold the incumbent electorally accountable for the quality of this outcome.



(a) Solid lines reflect the predicted probability with 95 pct. confidence intervals of voting for the mayoral party across satisfaction with unemployment services in treatment and control municipalities. Predictions are derived from a linear probability model with a treatment by unemployment services interaction. Standard errors are clustered at the municipal level. Interaction estimate is statistically significantly different from zero ($t(1521) = 2.08, p = 0.04$). Dots are conditional probabilities estimated from the sample ($n = 1522$). The dashed line in the “Treatment” plot reflects the slope from the “Control” plot.



(b) Solid lines reflect the predicted probability with 95 pct. confidence intervals of voting for the mayoral party across satisfaction with elderly care services in treatment and control municipalities. Predictions are derived from a linear probability model with a treatment by elderly care services interaction. Standard errors are clustered at the municipal level. Interaction estimate is not significantly different from zero ($t(1521) = 0.44, p = 0.44$). Dots are conditional probabilities estimated from the sample ($n = 1522$). The dashed line in the “Treatment” plot reflects the slope from the “Control” plot.

Figure 4.1 Does the mayor’s power over unemployment services condition the extent to which the mayor is held electorally accountable for the quality of these and other services?

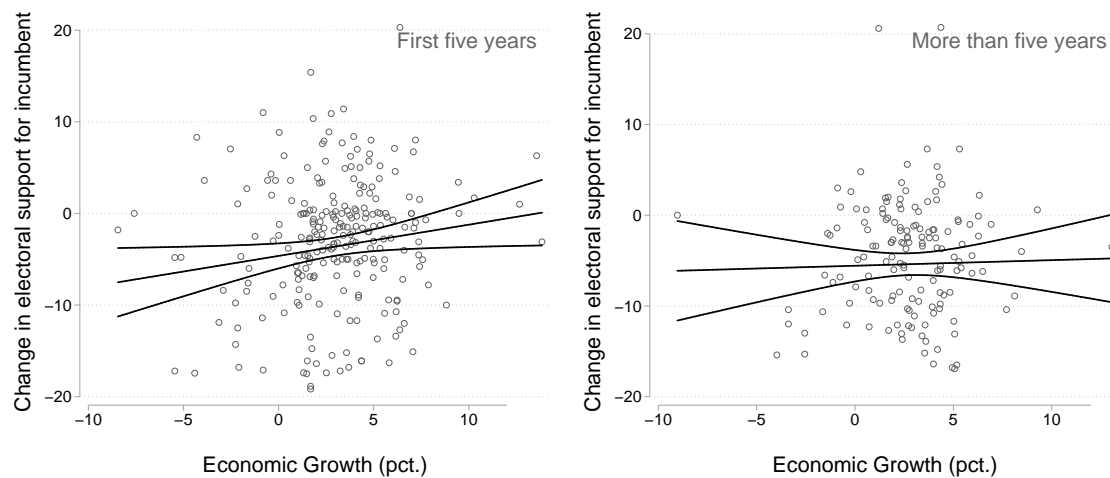
Incumbents who have been in office longer are held less electorally accountable for recent economic conditions

What is the relationship between incumbent tenure and retrospective voting? Intuitively, incumbents with more tenure should have had more time to enact policies that affect policy outcomes, and as such, following the clarity of responsibility hypothesis, they should be held more electorally accountable for these policies. However, this intuition is complicated by theories of Bayesian learning. Bayesian learning suggests that when a person's beliefs are weak, new evidence will have more of an impact than when their beliefs are strong. Since voters' beliefs about an incumbent are likely to strengthen as this incumbent's term proceeds, theories of Bayesian learning would predict that voters' beliefs about the incumbent would be more malleable to the quality of policy outcomes at the beginning of an incumbent's time in office.

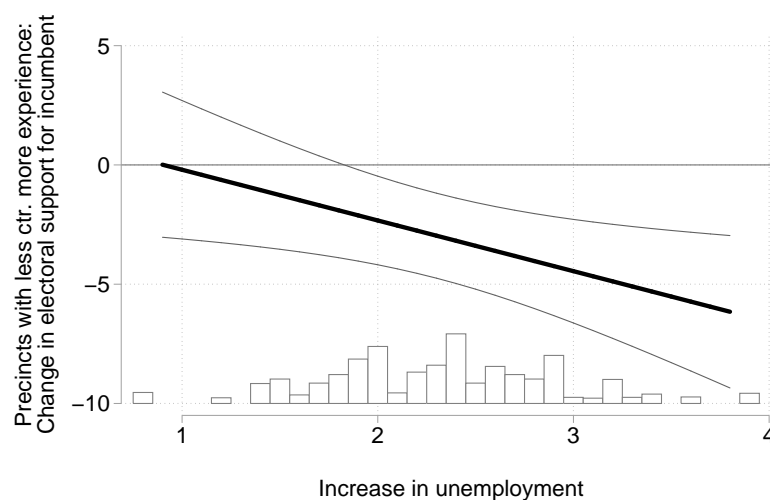
As an incumbent's time in office increases, clarity of responsibility might thus increase the incentive for voters to hold the incumbent accountable for the quality of policy outcomes while, at the same time, Bayesian learning might reduce this incentive. To find out which of these countervailing forces dominate, article C, 'Incumbent Tenure', conducts an extensive empirical investigation of the long-term relationship between incumbent tenure and retrospective voting.

The article begins by examining a dataset that covers country-level election returns from 409 elections in 41 different countries. Measuring economic retrospective voting as the correlation between economic conditions (i.e., economic growth) and support for the executive party, the article finds that as incumbent tenure increases, economic voting decreases. This finding is illustrated in figure 4.2, which compares the relationship between growth and change in electoral support for incumbents running for reelection in the first five years of their time in office with incumbents running for reelection after the first five years.

The article examines the robustness of this finding in a number of ways, introducing various controls, alternative estimation strategies, etc. Most importantly, the article conducts a conceptual replication, which also identifies a negative relationship between time in office and economic voting. The conceptual replication utilizes a pooled cross-section of 60 representative national surveys from 10 Western European countries, measuring the economic vote as the correlation between voters' retrospective evaluations of the national economy and their intention to vote for the executive party.



(a) Relationship between change in electoral support and economic growth for executive parties running for reelection. The plot on the left shows the relationship for executive parties running for re-election the first five years after getting into office ($n = 248$). The plot on the right shows the relationship for executive parties running for re-election after more than five years ($n = 155$). The lines reflect the expected support with 95 pct. confidence intervals from linear regression of economic growth on changes in electoral support. The dots represent elections. There is a statistically significant linear interaction between time in office and economic growth, cf. chapter 8.



(b) Predicted differences in electoral support for the mayoral party between the precincts that had the same incumbent before and after the municipal reform and the precincts that did not, across changes in the municipal unemployment rate with 90 pct. confidence intervals. See chapter 8 for estimation details. Histogram shows the distribution of increases in unemployment. Unemployment increased in all municipalities, because elections were held one year after the great recession in 2008.

Figure 4.2 What effect does the tenure of the executive party have on the extent to which the incumbent party is held electorally accountable for economic conditions? Evidence from a country-level dataset of elections (a), and precinct-level election returns from the 2009 municipal elections in Denmark (b).

Having established that there is a negative long term relationship between incumbent tenure and retrospective economic voting, the article zeroes in on the proposed theoretical mechanism underlying the negative relationship: Bayesian learning. In particular, the article focuses on a dataset of local elections in Denmark; local elections that, as described in chapter 3, were held in the wake of a large municipal redistricting reform. The article takes advantage of the fact that, as a consequence of the reform, a number of municipalities were amalgamated, which created within-municipality differences in the amount of experience the electorate had with the same incumbent mayoral party. Analyzing the election returns, the article finds that voters who had *less* experience with an incumbent mayor were *more* likely to hold the mayor electorally accountable for recent changes in local levels of unemployment. This result is presented in panel (b) of figure 4.2.

Taken together, the analyses conducted in the article paint a uniform picture: as incumbent tenure increases, retrospective (economic) voting decreases. This is especially interesting because it goes against the clarity of responsibility hypothesis, which predicts that incumbent tenure should stimulate, not depress, retrospective voting. In terms of the dissertation's research question, this means that, in some instances, governments will actually be held less electorally accountable for the current state of policy outcomes as their power over these outcomes increases. In particular, it seems to be the case that if increases in power over policy outcomes are accompanied by another factor (in this case a growing stock of prior experiences) which makes policy outcomes less informative as to the incumbent's type, then voters will not hold the incumbent more electorally accountable. Put differently, governments' power over policy outcomes might only affect the extent to which voters hold the government accountable when this power makes a difference in terms of how easy it is for voters to identify whether the government is acting in the voters' best interests. That is, power is only important to voters when it reveals. This is an important corrective to the clarity of responsibility literature in general and to article A in particular.

Incumbent Behavior

Governments' power over policy outcomes shapes voter behavior, but does this change in voter behavior also lead to changes in how incumbent governments behave? Since the goal of this dissertation is to understand how governments' power over policy outcomes shapes patterns of electoral accountability, and since electoral

accountability is fundamentally about the extent to which voters can use elections to constrain incumbent politicians, this is an important question to address. In spite of this, few existing studies have tried to engage with this question. The dissertation tries to do this in article B, 'Incumbent Behavior'.

Increasing government power over policy outcomes affects the type of policies pursued by incumbent governments

Since governments' power over policy outcomes moderates electoral accountability, it is only natural to assume that it will also moderate incumbent behavior by changing the type of incumbents that are elected, and the re-election incentives these incumbents face once they are elected. To see this, imagine that governments have extensive power over policy outcomes, so that voters hold them very electorally accountable for the state of these outcomes. In this case, governments who are (not) able to produce the type of policy outcomes that voters prefer should stand a better (worse) chance of getting re-elected (i.e., a selection effect), and governments who are not immediately willing to obtain the type of policy outcomes voters prefer get an extra electoral incentive to do so (i.e., a sanctioning effect). Conversely, when governments have little power over policy outcomes, their re-election chances will not depend as strongly on the policy outcomes they produce, minimizing their incentive to obtain the type of policy outcomes voters prefer, and maximizing the probability that incumbents who are not willing or able to produce the type of outcomes voters prefer will be re-elected. Following this, we might expect that as governments' power over policy outcomes increases, so will the frequency and intensity with which incumbent governments pursue the type of policy outcomes that their electorate prefers.

In article B, 'Incumbent Behavior', which is co-authored with Asmus Leth Olsen, we explore this theoretical expectation by studying income tax policy in Danish municipalities. We leverage three important aspects of this context:

1. Local income tax increases are unpopular with the average voter, and can therefore be used as a metric of whether incumbent politicians act in accordance with the interests of local electorates.
2. When the largest party controls a majority of the seats, this party can set the municipal income tax rate without coalescing with other parties. Single party majority status can therefore be used as an indicator of the largest party's power over policy outcomes.

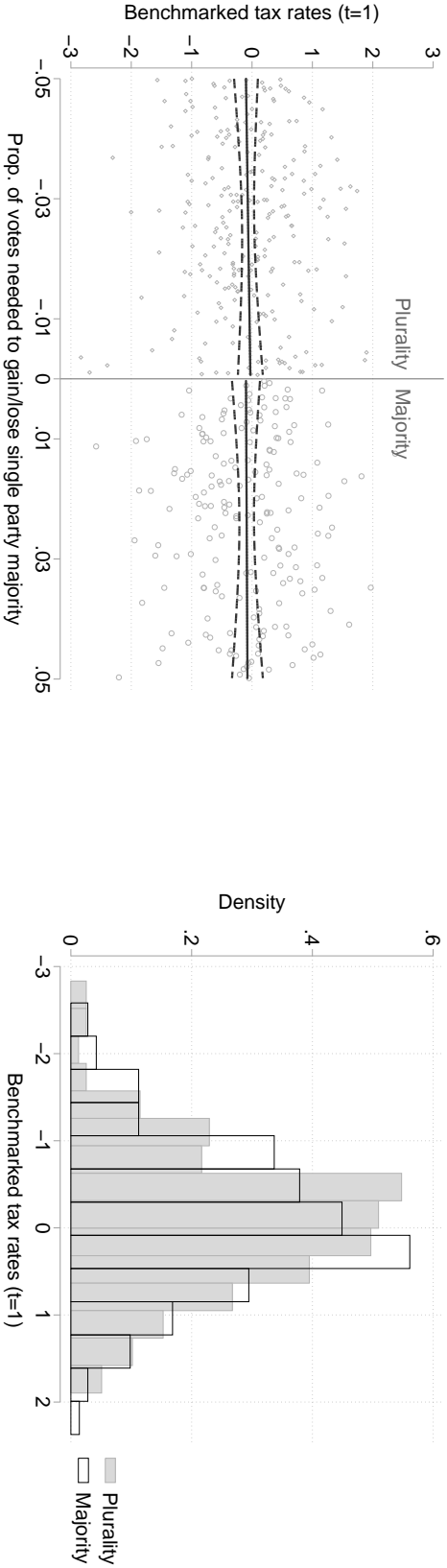
3. Assignment to single party majority status is the result of an electoral discontinuity, and can therefore be used to identify the causal effect of single party majority status on income tax policy around the discontinuity (cf. chapter 3).

We lay out these different aspects of the Danish context at length in the article, however the assertion that local income tax policy can be used as a metric of whether incumbent politicians act in accordance with the electorates preferences might need some additional justification up front. Municipal income taxes in Denmark are broad-based, substantial in magnitude, and highly salient. Further, previous literature has shown that income taxes are unpopular with the average voter: if a municipality raises taxes, the mayor generally loses electoral support (Bhatti, Hansen and Olsen, 2012; Frandsen, 1998). This is a finding we also replicate in our article. Accordingly, it seems reasonable to assume that, other things being equal, the electorate would prefer municipal income tax rates to be lower.

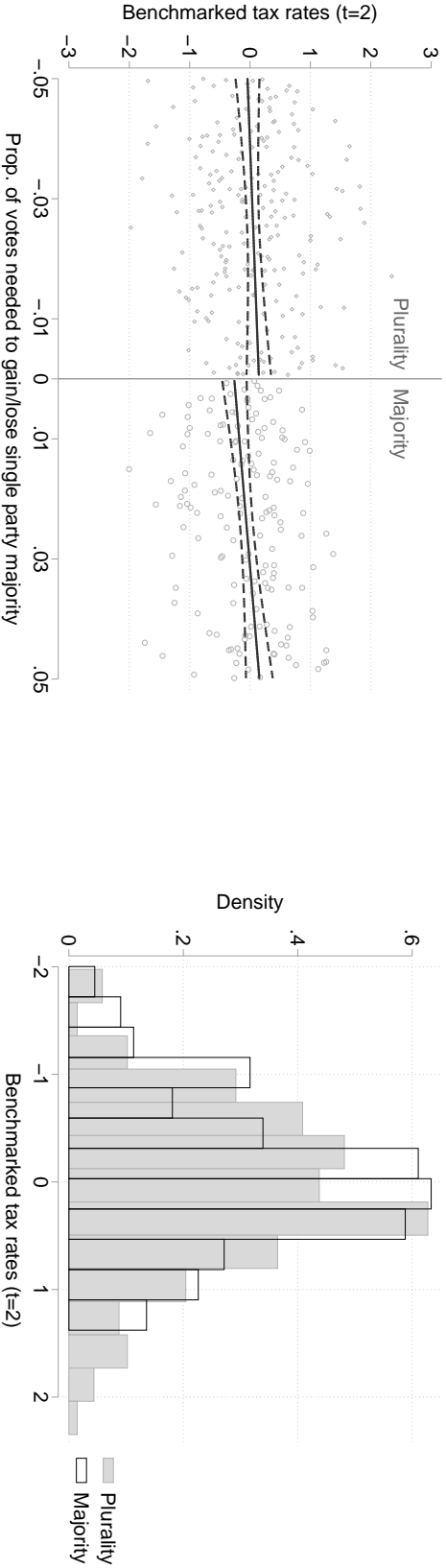
Figure 4.3 presents the central findings from our article. As can be seen from this figure, there is no evidence of an immediate effect of single party majority status on tax rates, but there is evidence of a long-term effect. As such, whether or not a party wins a single party majority, does not seem to have an effect on the tax rate set at the next election ($t=1$), but there does seem to be a negative effect of about half a percentage point if one looks at the subsequent election ($t=2$). In particular, if we look at the distributions of tax rates, we can see that there are almost no municipalities with high benchmarked tax rates (> 1) among single party majorities at this latter election. This tells us that when the largest party in the city council gets a single party majority, increasing this party's power over tax policy, there is no evidence that this leads the largest party to seek lower taxes. However, if the largest party has a majority of the seats going into an election, then the type of city-council that gets elected will be more prone to seek lower taxes.

The selection-mechanism, described in detail in chapter 2, might explain this delayed effect. As such, if the largest party holds a majority of the seats, it is easier for voters to identify and reduce the political influence of parties that are not interested in lowering taxes, and, by extension, they are able to empower parties who are able and willing to lower taxes. In the article, we provide additional evidence for this mechanisms, showing that parties who have a single party majority are in fact held more electorally accountable for their tax policy.

These results go nicely with what was found in article A; namely that voters' mode of selection is affected by their government's power over policy outcomes. However, the results also go beyond confirming what we found in article A, because



(a) Tax rates when largest party has a plurality or majority.



(b) Tax rates after the largest party has had a plurality or majority.

Figure 4.3 How does single party majority status affect the tax rates adopted by the city council? Relationship between forcing variable and residualized tax rates for elections in which single party majority status was determined by less than 5 pct. of the vote. See chapter 7 for details.

we can now see that this change in voter behavior also affects the type of policies pursued by local incumbents.

Voter Attributions

So far, this chapter has discussed how governments' power over policy outcomes affects voter and incumbent behavior. Now it delves into whether and how governments' power over policy outcomes affects the way voters attribute political responsibility.

Ambiguity about the causes of policy outcomes can create a self-serving bias in attribution of political responsibility

As described in chapter 2, a number of studies have found that voters who identify with or feel close to the government party attributes responsibility selectively, believing the government to be more (less) responsible for economic outcomes if these outcomes are good (bad). This pattern of behavior reveals an instinct, among some voters, to adjust their perceptions of who is responsible based on what makes their preferred party look good. This type of 'directional' thinking (Taber and Lodge, 2006), where political cognition is adjusted to arrive at predetermined goals, might interact with ambiguity about a government's role in shaping policy outcomes to create a self-serving bias in attribution of political responsibility.

To see this, imagine that a voter loses his or her job. Arguably, this is, in part, a result of policies enacted by the national government; however, it is also, in part, a result of the voter's own behavior. If voters want to make themselves look better, then they are motivated to over-emphasize the role the government played in getting them laid off, and under-emphasize the role they played themselves. That is, a self-serving motive leads voters to hold governing politicians more responsible for policy outcomes if these outcomes are negative than if they are positive. Importantly, this self-serving bias will only kick in if voters can plausibly assign responsibility to themselves for the policy outcome. If they cannot do this, then voters' self-image will not be affected by the result of the attributional process, and there is no role to play for a self-serving bias.

Article D, 'Self-servingness', examines whether there are signs of such a self-serving bias in the way voters' attribute political responsibility. One of the article's approaches to this question is a vignette survey experiment on a population-based

sample of Danish voters. In the survey, respondents are asked to evaluate the extent to which the government would be responsible for a set of hypothetical outcomes related to either housing or (un)employment. The valence (positive, negative or neutral) of the outcome, and whether voters can reasonably assign responsibility themselves for the outcome, varies randomly across respondents. Whether voters can assign responsibility to themselves for the outcome is manipulated by presenting voters with outcomes at either a personal or at a national level.

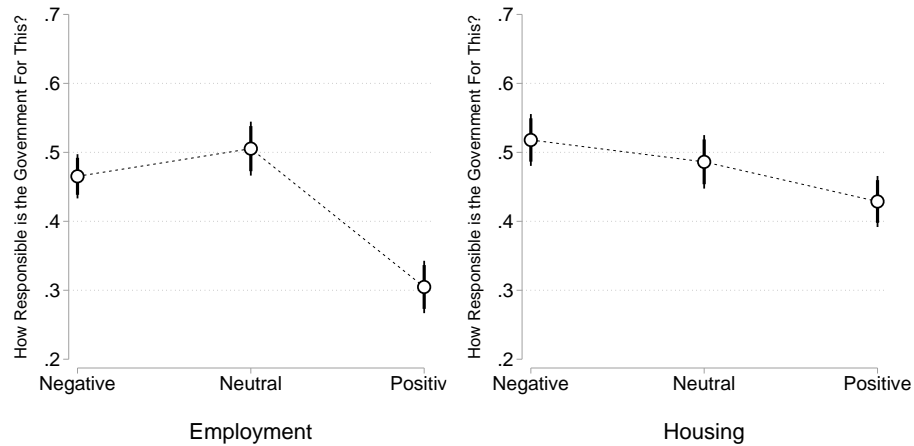
Figure 4.4 presents the results from the experiment. In line with what we would expect if there is a self-serving bias in political attribution, the figure shows that if respondents can assign responsibility to themselves for an outcome then they are more likely to hold the government responsible for the outcome if it is negative than if it is positive if the respondent is in panel (a). There is no such asymmetry for the national outcomes in panel (b).²

In addition to this, the article looks for signs of a self-serving bias in a number of election surveys (i.e., the Danish National Election Studies, the American National Election Studies and the Latinobarómetro). Consistent with the self-serving bias in attribution, the article finds that participants in these election surveys who think their *personal* economic situation is improving are no more or only slightly more likely to support the incumbent government than voters who think their economic situation has stayed the same, while those who think their personal economic situation is worsening rather than stagnant are markedly less likely to support the incumbent. The article does not identify a similar valence asymmetry in support for the incumbent across evaluations of the *national* economic situation. Finally, the article utilizes a population-based survey of Danish voters, asking respondents to evaluate the extent to which the government can affect their personal economic situation, and look at how these correlate with voters' evaluations of their personal economic situation. The article finds that the voters who believe their own economy is doing better are less likely to believe the government can affect this.

Taken together, the different analyses in article D all point to the existence of a self-serving bias in attribution of responsibility, which is driven by voters' uncertainty about the extent to which the government, and they themselves are responsible for certain policy outcomes.

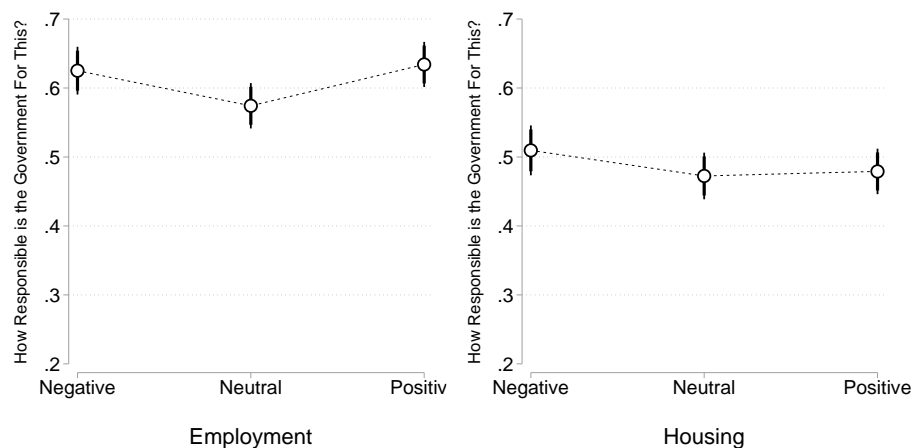
²The article conducts a second survey experiment with a convenience sample to validate the results from this experiment.

Personal Economic Conditions



(a) Voters' beliefs about government responsibility for randomly assigned hypothetical economic outcomes ($n = 1,002$). Vertical lines are 95 (thin) and 90 (thick) pct. confidence intervals. See chapter 9 for details. Housing outcomes: "Imagine that the price of your or your family's house [increased/decreased/increased or decreased]." Employment outcomes: "Imagine that you or someone in your family [lost their job/got a better job/lost their job or got a better job]."

National Economic Conditions



(b) Voters' beliefs about government responsibility for randomly assigned hypothetical economic outcomes ($n = 1,002$). Vertical lines are 95 (thin) and 90 (thick) pct. confidence intervals. See chapter 9 for details. Housing outcomes: "Imagine that the price of houses in the country as a whole [increased/decreased/increased or decreased]." Employment outcomes: "Imagine that unemployment in the country as a whole [increased/decreased/increased or decreased]."

Figure 4.4 Is there a self-serving bias in attribution of political responsibility?

Chapter 5

Conclusion

This chapter presents a cohesive answer to the research question presented in chapter 1, summarizing the key theoretical and empirical arguments made in the rest of the dissertation. Broadly speaking, the chapter concludes that a key impediment to electoral accountability is that power over policy outcomes is often spread out over several disparate political actors. When this is the case, it is hard for voters to figure out who is responsible, and, in turn, who to hold accountable for the quality of policy outcomes. As a result, voters will not let their decision to re-elect incumbents depend on the state of policy outcomes and incumbents will be less inclined to pursue the type of outcomes voters prefer. The chapter also discusses potential implications of this conclusion for institutional design and voter competence.

Power Reveals

One of the most widely replicated findings in political science is that electoral support for incumbent politicians depend on incumbents' ability to provide a good standard of living for their citizens. This type of voter behavior is called retrospective voting, because it implies that voters focus on how politicians have performed in the past. Retrospective voting carries with it the promise of electoral accountability. Indeed, if citizens only reelect governing politicians when social and economic conditions are improving, they make sure that re-election minded politicians have an incentive to pursue such improvements, and they make sure that politicians who are not able (or do not care) to pursue them get voted out of office. This link between retrospective voting and electoral accountability, however, is premised on politicians' ability to leave a clear mark on the societies they govern. If politicians do not have this ability, retrospective voters end up holding politicians accountable for developments they had little or no responsibility for.

At the same time, it seems clear that incumbent politicians' power to make a difference for their voters varies substantially across and within democracies. For instance, in some cases, a single party can form a government alone, and in others they have to collaborate with other parties. In some cases, the responsibility for administering national programs lies with the national government alone, and in others the national government has to work with local officials.

Taking these considerations as a starting point, the goal of this dissertation has been to figure out whether and to what extent variation in governments' power over policy outcomes shapes patterns electoral accountability.

Through a series of research articles, the dissertation has come to the conclusion that electoral accountability is concomitant with centralization of power over policy outcomes in the hands of the government. In particular, the dissertation has found that when an incumbent government gets more power over policy outcomes, voters let their decision to re-elect this government depend more strongly on the state of these outcomes, which then affects the extent to which the government tries to align the state of policy outcomes with what the voters prefer. A change in incumbent behavior which seems to be driven entirely by the fact that voters are more likely to elect politicians that act in voters' best interest, when governments' power over policy outcomes increases (cf. article B).

It is rational for voters to act in this way. As such, when the government's power over a policy outcomes increases it becomes easier for voters to identify whether

the government is aligning the state of this policy outcome with what voters prefer. Conversely, when the government's power over a policy outcome decreases, this policy outcome become less informative as to how the government behaved in office, and accordingly, a less relevant metric to judge them by. Put differently, when governing politicians have the power to change the life of the voters for the better or for the worse, it is easier for voters to know whether they are — in fact — doing so. In this way, power reveals to the voters what kind of politician they are dealing with, and voters act on this revelation.

This conclusion is, broadly speaking, consistent with the clarity of responsibility literature presented in chapter 2, and the dissertation can thus be said to have provided additional evidence for the clarity of responsibility thesis. In particular, the dissertation has provided more methodologically rigorous tests of empirical implications already laid out in the existing literature (e.g., whether clarity of responsibility plays a causal role in shaping electoral accountability), and explored hitherto untested empirical implications (e.g., implications for incumbent behavior). In addition to this, the dissertation has also painted a more nuanced picture of the relationship between governments' power over policy outcomes and electoral accountability, than the one put forward in the existing literature.

For one, the dissertation has found that the relationship between responsibility and accountability works at the level of individual policy outcomes. That is, voters are able to differentiate between the extent to which the same government has different levels of power over different policy outcomes, and hold the government accountable accordingly (cf. article A). If, for instance, an incumbent government is given more power over a specific policy, then voters will hold the government more accountable for the outcomes of this policy, but not for the outcome of other policies. This is potentially quite important, because it means that to the extent that each political actor is responsible for a clearly demarcated set of policy areas, power sharing across political actors does not, necessarily, attenuate electoral accountability. More on this in the next section.

Further, while it is rational for voters to hold their government less electorally accountable as the government's power over policy outcomes decrease, there are also other, less rational, forces which attenuate electoral accountability as power over policy outcomes diffuses. As such, the dissertation found that when government's power over a policy outcome is shared with the voters themselves (e.g., power over voters' personal economic situation), voters attribute responsibility for this outcome in a self-serving way. In particular, to the extent that voters can reasonably hold

both themselves and the government responsible, they tend to hold the government more responsible for undesirable outcomes (thus exculpating themselves) and less responsible for desirable outcomes (thus implicating themselves). Compared to a rational benchmark, voters can thus be expected to reward politicians too little, and punish them too much, for outcomes that are the result of government intervention as well as an individual's own behavior (cf. article D).

Finally, the dissertation found that as incumbent governments' time in office increases, voters' inclination to hold these incumbents accountable for the present state of the economy decreases (cf. article C). This runs counter to the clarity of responsibility thesis, because when incumbents' time in office increases, so does their power to shape the current state of the economy. Even so, the negative relationship between economic voting and incumbent tenure makes sense, if voters only act on their government's power over policy outcomes to the extent that more power means that policy outcomes become more informative as to the government's type (i.e., whether the government is willing and able to act in voters' best interest). As such, while governments' ability to influence the current economic situation increases with time in office, the overall informativeness of the current economic situation might very well decrease over time, because voters stock of information about the government's type also increases with time in office, crowding out the effect of power over policy outcomes. In essence, this finding suggests that power only matters when it reveals.

Implications for Institutional Design

If we are interested in reinforcing electoral accountability, then a straightforward implication of the conclusions laid out above is that we should prefer political institutions that centralize power in the hands of few(er) political actors. This type of centralization will make it easier for voters to identify who is responsible for policy outcomes, and, in turn, make it easier for voters to reject elected officials who seek, or who are not able to avoid, undesirable outcomes. If we opt for institutions which disperse power across a multitude of actors (some of whom might not be up for election at all), it is hard for voters to know who is responsible for any undesirable outcome, increasing the chances that elected officials who are unable or unwilling to align policy outcomes with the electorates' preferences are left in power by the clueless, in the sense of not having been provided with a clue, voters.

This dissertation is not the first to make this type of recommendation in terms of institutional design. (See, for instance, the Alexander Hamilton quotation which introduced chapter 1.) In particular, the American good government movement of the late 19th and early 20th century had centralization of power as a central tenet of their reform program (Caro, 1974, 59-170). One of the founders of this movement, Woodrow Wilson, expressed the virtues of centralized power as follows:

“And let me say that large powers and unhampered discretion seem to me the indispensable conditions of responsibility. Public attention must be easily directed, in each case of good or bas administration, to just the man deserving of praise or blame. There is no danger in power if it only be responsible. If it be divided, dealt out in shares of many, it is obscured, it is made irresponsible” – Wilson (1887, 213)

This idea was powerful enough to have important consequences for institutional reform in the American States. As such, when the New York constitution was reformed in the beginning of the 20th century the main thrust of the reform was centralization of power in the hands of the governor. In fact, the centralization was proposed as a response to concerns that voters were not be able to hold the current state government accountable. This is clear from the Reconstruction Commission’s – which proposed the new constitution – report, which argued that: “It is clear that if New York wants retrenchment and efficient government it must make some one responsible who can be held to account and give him power commensurate with his obligations. There is no other way.” Alfred E. Smith, the governor at the time, was even more direct in his preface to the report: “The people must give the Governor authority if they want to hold him responsible.”¹

The findings from this dissertation suggests that the assertion made by these good government reformers were correct, and, to the extent that responsibility can be understood as electoral accountability, the Wilson quote succinctly sums up this dissertation’s main conclusion: Large powers and unhampered discretion are indispensable conditions of accountability.

That institutions which centralize power over policy outcomes has a democratic edge as opposed to those that disperse power is somewhat controversial in the modern comparative politics literature. Most notably, Lijphart (1984, 1999) who groups democracies into consensual democracies, where power is shared, and majoritarian democracies, where power is more centralized, suggests that “the overall

¹The report, including the governor’s preface can be found at <https://archive.org/details/cu3192401400510>

performance record of the consensus democracies is clearly superior to that of the majoritarian democracies, [and] the consensus option is the more attractive choice for countries designing their first democratic constitutions or contemplating democratic reform” (Lijphart, 1999, 296).

This is the exact opposite recommendation from the one derived from this dissertation. However, Lijphart comes to this conclusion by looking at a different set of indicators than electoral accountability. As such, it might be the case that, on some of indicators of the quality of democracy, centralizing power has adverse effects, while, at the same time, it reinforces electoral accountability. For one, Lijphart suggests that power-sharing might ease tensions in countries with deep religious and ethnic division (Lijphart, 1984, 295), by making sure that all groups are represented, to some extent, in the countries’ government. A conclusion which seems reasonable.

It is outside the scope of this dissertation to examine Lijphart’s claims in detail or to look at whether increasing governments’ power over policy outcomes affects other aspects of democratic quality than electoral accountability. However, if any adverse effects exist, then it might be worthwhile to consider whether the findings in our dissertation might allow for some form of power sharing arrangement, which does not dilute electoral accountability.

In research article A, we found that when some local governments in Denmark became more responsible for unemployment services, voters were able to recognize this and held local officials more accountable for the quality of unemployment services, but no more responsible for the quality of other services. As mentioned above, this suggests that voters are able to hold different layers of government accountable for different policy outcomes, as long as power for specific policies is not shared across the different layers of government. That is, in a system where local or provincial governments control one set of policies (e.g., provision of public services) while national governments control another set of policies (e.g., infrastructure and national defense), voters should be able to hold officials at all levels accountable for the policy outcomes they each have power over.

While this institutional set-up might be away to get at power sharing without attenuating accountability, there is probably a limit to how much power sharing is viable if one wants to keep the institutions comprehensible. The enormous web of special purpose jurisdictions in the United States, for instance, probably obscure accountability even though each jurisdiction, for the most part, only has responsibility for one policy outcome (Berry, 2009). Even so, in countries like Denmark, where local and national politicians share power in a number of important policy areas, such as

provision of most public services, a more clear demarcation of who is responsible for what, might very well increase electoral accountability, without changing the overall distribution of political power (across policy outcomes) between the local and the national level.

Implications for Voter Competence

The main hindrance to effective accountability identified in this dissertation is not that voters are incompetent. In fact, this dissertation suggest that voters act in quite sophisticated ways when assigning credit or blame for policy outcomes, recognizing and acting on the extent to which government has power over specific policy outcomes. Indeed, the main culprit when it comes to the lapses in accountability is not voters themselves but institutions which attenuate the relationship between the behavior of elected officials and the quality of important policy outcomes.

This goes against parts of the existing literature which suggest that voters perceptual and attributional biases are so severe that they can never hold politicians effectively accountable (e.g., Achen and Bartels, 2016). Instead, this dissertation suggests, that with the right set of institutions, electoral accountability is a real possibility. Even so, the findings in this dissertation *does not* challenge the existence of perceptual or attributional biases in accountability writ large. There is no contradiction between the the presence of meaningful differences in the strength of electoral accountability across contexts, and the presence of psychological biases which attenuate accountability across contexts by, for instance, making voters' overemphasize recent performance (e.g., Healy and Lenz, 2014).

Yet it might seem odd that voters can be led astray by perceptual and attribution biases, while, at the same time, making and acting on fair assessments of their government's power over policy outcomes. In relation to this, it is important to note, that this dissertation has primarily focused on how voters act in the aggregate, rather than how individual voters attribute responsibility.² Therefore, this dissertation can only speak to whether the aggregate electorate, not the individual voter, is able to hold incumbents accountable in proportion to how implicated they are. This is an important distinction, because to the extent that voters actions are mediated by the behavior of political elites, our findings are consistent with individual voters having

²Even in articles A and C, where we use individual-level data, we do not directly examine the the extent to which voters find the government to be responsible for policy outcomes. In fact, when we look at how individuals attribute responsibility, as we do in article D, we find that they fall victim to a psychological bias.

little to no information about the distribution of power over policy outcomes. If, for instance, incumbents, challengers and the media focus their political attention on outcomes the government has significant power over, then this might direct voters (electoral) attention to these outcomes as well, creating a relationship between government's power over policy outcomes and the extent to which this government is held electorally accountable for the quality of these policy outcomes.

It is beyond the scope of this dissertation to identify whether the importance of governments' power over policy outcomes is, in fact, mediated by the behavior of political elites, or by individual voters ability to correctly identify and act on who has the power to shape specific policies. Instead, the point I want to make is simply that the findings in this dissertation do not have strong implications for voter competence at the individual level, and the findings should therefore not be seen as a repudiation or corrective to studies documenting limits to the knowledge and capacities of individual voters.

A Note on The Dissertation's Scope of Inference

This dissertation has primarily, though not exclusively, relied on empirical observations of how politicians and voters behave in the context of local elections in Denmark. Naturally, this means that one needs to be careful about drawing broad implications, at least in the absence of corroborating evidence from other parts of the world as well.

Such evidence is, in part, present in the clarity of responsibility literature discussed in chapter 2, but, as was also discussed in that chapter, there are limits to what conclusions one can draw from the existing literature, making it unclear whether it really counts as corroborating evidence. For instance, it was unclear whether the effect of power-sharing institutions on the extent of electoral accountability was causal (see also article A), and it was unclear whether the presence of these institutions affected the type of policies pursued by incumbent politicians (see also article B). As such, we cannot know for sure whether the original results travel outside the context(s) studied in this dissertation, until future studies find, or do not find, corroborating evidence from other contexts.

Even though it is impossible to reach any definite answers with respect to the scope of inference of the dissertation, it might still worthwhile to think about whether there are any features of the context studied here which inherently delimit the scope. The most important feature in this regard is arguably that the Denmark is a consolidated

democracy, where most political actors respect certain democratic norms. At the same time, while the variation in Danish local governments' power over policy outcomes studied in this dissertation is substantial, the most powerful local government still has severe limits on the extent of their power. These features of the Danish system neutralize a potential concern that one might have in relation to centralizing power over policy outcomes, namely, that if power becomes too concentrated, then the government might be able to manipulate voters perception of the state of policy outcomes by censoring the press, or, even worse, infringe upon the integrity of the electoral process. Accordingly, if one wants to reinforce electoral accountability, there probably is a limit to how much one would want to concentrate power, and this limit might be lower in countries where breaches of democratic norms are less consolidated. However, a large number of solidly democratic political systems should be able to centralize power, without becoming vulnerable to such threats.

Research Articles

Chapter 6

Is the Relationship between Political Responsibility and Electoral Accountability Causal, Adaptive and Policy-specific?

Will voters hold an incumbent more electorally accountable for the quality of a policy outcome if the incumbent's political responsibility for the underlying policy increases? To answer this question, this study exploits a reform of labor market regulation in Denmark that exogenously assigned more political responsibility for unemployment services to some municipal mayors. The study finds that in subsequent elections these mayors were held more electorally accountable for unemployment services, but not more accountable for other policy outcomes. This suggests that the relationship between political responsibility and electoral accountability is causal, adaptive and tied to specific policies. On balance, the electorate thus seems to be quite judicious when assigning electoral credit or blame, moderating the extent to which incumbents are held accountable for specific outcomes based on the extent to which these incumbents crafted and implemented the policies that shaped these outcomes.

Voters hold incumbent politicians accountable for how they perform in office by making electoral support for these incumbents contingent on the quality of policy outcomes (for a recent review, see Healy and Malhotra, 2013). These outcomes include economic conditions (Kramer, 1971; Lewis-Beck, 1990; Van der Brug, Van der Eijk and Franklin, 2007), public service provision (Burnett and Kogan, 2017; Tilley and Hobolt, 2011), plus management of other problems which might arise during an incumbent's time in office (e.g., handling of natural disasters, cf. Healy and Malhotra, 2010). While this type of electoral accountability is widespread, it seems to be more prevalent in some elections than it is in others (Duch and Stevenson, 2008; Van der Brug, Van der Eijk and Franklin, 2007). In particular, a large number of studies have found that the extent to which a political system concentrates or disperses political responsibility, what is conventionally called 'clarity of responsibility', correlates with the extent to which voters hold governing politicians electorally accountable for the quality of policy outcomes (Duch and Stevenson, 2008; Powell and Whitten, 1993; Whitten and Palmer, 1999).

This correlation between responsibility and accountability is strong and consistent across many different indicators of clarity of political responsibility. For instance, studies have shown that voters are less likely to hold incumbents accountable for the quality of the economic situation if the incumbent operates in an open economy (Fernández-Albertos, 2006; Hellwig and Samuels, 2007; Hellwig, 2001), if the incumbent is part of a coalition government (Hobolt, Tilley and Banducci, 2013; Nadeau, Niemi and Yoshinaka, 2002), or if the incumbent operates in a federal system (Anderson, 2006; Cutler, 2008; León, 2011).

Broadly speaking, these studies have been used to infer that electoral accountability will "strongly reflect the nature of policymaking in the society and the coherence and control the government can exert over that policy" (Powell and Whitten, 1993, 398). While this does tell us something important about how political responsibility relates to electoral accountability, it does not tell us everything.

For one, it is not clear whether the correlation identified in the previous literature reflects a *causal* relationship between responsibility and accountability. We know from the political economy literature that political institutions, like those dispersing or concentrating political responsibility, are fundamentally endogenous (Acemoglu, 2005; Besley and Case, 2000). Accordingly, it is possible that the observational co-occurrence between responsibility and accountability is partly or completely driven by some extraneous, underlying factor. Further, because previous literature has primarily focused on institutional differences in political responsibility that were

established a long time ago, we do not know exactly how *adaptive* the relationship between electoral accountability and political responsibility is. That is, we do not know whether voters are quick to adjust to changes in political responsibility, or whether the correspondence found in previous research is the result of a long-term process that slowly adjusts levels of accountability to levels of responsibility. Finally, we know little about the extent to which the relationship is *policy-specific*. That is, whether voters recognize and act on differences in the extent to which the same incumbent is responsible for individual policy outcomes (e.g., unemployment ctr. inflation). We do not know this because previous research has primarily examined institutional differences that implicate changes in incumbents' responsibility for a wide range of outcomes (e.g., federal contra unitary constitutions), making it impossible to discern whether voters are sensitive to policy-specific differences in political responsibility. Causality, adaptiveness and policy-specificity are important features of the relationship between responsibility and accountability, because they all tell us something about the extent to which voters only hold incumbent politicians electorally accountable for the policy outcomes the politicians had a hand in shaping, and thus whether voters are able to use elections to select politicians who can competently manage the policies they are responsible for.

This article re-examines the relationship between political responsibility and electoral accountability in a context where it possible to cast some light on these different and largely unexplored features of the relationship between accountability and responsibility: a reform of labor market regulation in Denmark. The reform increased municipal mayors' responsibility for unemployment services, and *only* for unemployment services, by making the municipalities responsible for the administration of active labor market policies. In 14 municipalities, the reform was implemented three years before the 2009 municipal elections, and in the remaining 84 municipalities, it was implemented after these elections. The decision about which municipalities had to implement the reform before the 2009 election was taken practically unilaterally by the central government, and a closer examination of the selection process reveals that the central government plausibly chose municipalities independently of pre-reform levels of electoral accountability. In sum, this reform presents a rare instance in which assignment of political responsibility for a specific policy outcome changed abruptly and exogenously, making it possible to examine whether the relationship between responsibility and accountability is policy-specific, adaptive and causal.

Using the Danish Municipal Election Survey (Elklit and Kjær, 2013), I show that voters in the municipalities where the labor market reform was first implemented, the treatment municipalities, held the mayor more electorally accountable for the quality of unemployment services in the election following the reform. This finding is robust to a number of different model specifications and a number of different estimation methods. Further analyses show that the voters in these treatment municipalities did *not* hold their mayor more electorally accountable for the quality of services unaffected by the reform. This immediate adjustment in electoral accountability for unemployment services, and only for these services, in response to an exogenous and recent change in political responsibility, suggests that the relationship between political responsibility and electoral accountability causal, adaptive and policy-specific.

This article extends the literature on how incumbents' political responsibility shapes voters' assignment of electoral credit and blame for policy outcomes – a literature which has, broadly speaking, remained observational and paid little attention to changes in political responsibility for discrete policy outcomes (e.g., Anderson, 2000; Duch and Stevenson, 2008; Harding, 2015; Hobolt, Tilley and Banducci, 2013; Powell and Whitten, 1993; Whitten and Palmer, 1999). Further, with a fairly consensual multi-party system (Houlberg and Pedersen, 2015), which garners less attention from voters than national politics (Elklit and Kjær, 2013), the Danish municipalities provide a hard case in a literature that has mostly focused on national politics, where sharp divides between opposition and government as well as higher levels of political attention potentially amplify the relationship between responsibility and accountability (De Vries et al., 2011; Duch and Stevenson, 2008). By demonstrating that voters are able to react in a reasonable way to a change in political responsibility from one level of government to another, this study also challenges the scope of research showing that voters have a hard time attributing responsibility in multi-level systems (Cutler, 2008; Johns, 2011; Sances, 2017). Instead, voters seem, on balance, to be quite judicious when assigning credit and blame for the quality of policy outcomes to local politicians.

Challenges in the Study of Responsibility and Accountability

When studying the relationship between political responsibility and electoral accountability, existing research has typically: (1) indexed different elections according

to how much political responsibility economic and political institutions assign to the incumbent up for reelection; (2) measured how accountable the incumbent was held in the same elections by correlating electoral support for the incumbent with a subjective indicator (e.g., perceptions of the national economy) or an objective indicator (e.g., economic growth) of the quality of policy outcomes; and (3) linked the responsibility index with the measure of electoral accountability in a statistical model. Using this approach, a number of scholars have explored the relationship between responsibility and accountability using different indices of incumbent responsibility (e.g., Anderson, 2006; Carlin and Singh, 2015; Duch and Stevenson, 2008; Hellwig and Samuels, 2007; Hobolt, Tilley and Banducci, 2013; Nadeau, Niemi and Yoshinaka, 2002; Paldam, 1991; Powell and Whitten, 1993; Whitten and Palmer, 1999), different policy outcomes (De Vries, Edwards and Tillman, 2010; Tavits, 2007; Tilley and Hobolt, 2011) and different types of elections (Berry and Howell, 2007; Ebeid and Rodden, 2006). Broadly speaking, these studies have found that in elections where incumbents have more responsibility for policy outcomes, they are also held more electorally accountable for the quality of these outcomes.¹

This wealth of thorough and innovative studies has gotten us a long way when it comes to understanding how political responsibility shapes electoral accountability. However, if one wants to draw more detailed inferences about the relationship between responsibility and accountability from the extant literature, one faces several challenges. In particular, based on previous studies, one would have a hard time evaluating whether political responsibility has a causal effect on electoral accountability, a hard time evaluating how quickly voters adapt to changes in political responsibility, and a hard time evaluating the extent to which voters are sensitive to differences in how politically responsible incumbents are for different policy outcomes. That is, one would have a hard time evaluating whether the relationship between responsibility and accountability is causal, adaptive and policy specific.

¹Another set of studies have examined which psychological processes lead voters to attribute certain outcomes to incumbent politicians (e.g., Gomez and Wilson, 2001; Tilley and Hobolt, 2011). While this literature also examines responsibility in relation to retrospective voting, it does so in a very different way than the literature discussed here. As such, in this more psychological literature, responsibility is a subjective belief voters hold, whereas in the literature described above, responsibility is an objective political condition determined by the mix of political and economic institutions that characterize the nature of policy-making in a specific polity.

Causality

Previous studies have almost exclusively analyzed the relationship between responsibility and accountability by looking at the correlation between the presence of institutions which manipulate incumbent responsibility and the extent to which voters hold incumbents electorally accountable (Duch and Stevenson, 2008). At the same time, however, most researchers agree that one can rarely estimate the causal effect of institutions using standard observational studies (Aghion, Alesina and Trebbi, 2004; Besley and Case, 2000; Meyer, 1995), because institutions are typically endogenous to the outcomes of interests (Acemoglu, 2005; Przeworski, 2004). Accordingly, it is possible that the relationship between responsibility and accountability is confounded.

This possibility looms large if one takes a close look at some of the specific institutions that have been used to get at the relationship between responsibility and accountability. For instance, a number of studies have shown that a country's economic openness is negatively correlated with how electorally accountable its executive is held for the economic situation (Duch and Stevenson, 2008; Fernández-Albertos, 2006; Hellwig and Samuels, 2007; Hellwig, 2001). These studies argue that this correlation is driven by the fact that economic openness decreases political responsibility for economic outcomes. However, economic openness is also known to be correlated with the extent to which countries provide social protection to those who are unemployed (Cameron, 1978; Rodrik, 1996), and we know from studies of economic voting, that economically vulnerable voters are more likely to punish and reward governing politicians for the state of the economy (Fossati, 2014; Pacek and Radcliff, 1995; Singer, 2013*b*). Accordingly, when researchers find that voters are less likely to hold their government electorally accountable for the economic situation in countries with an open economy, this might be because open economies have extensive social protections for their citizens, leaving citizens in these countries less worried about short-term fluctuations in the economy.

Another example of a potentially endogenous institution can be found in studies demonstrating that single party governments are more likely to be held electorally accountable for the economic situation than multi-party governments (Anderson, 2000; Hobolt, Tilley and Banducci, 2013; Nadeau, Niemi and Yoshinaka, 2002). This might be because it is harder to assess who is responsible for economic outcomes in a coalition government, but it might also be the result of another difference between coalition and single party governments. However, a number of studies have documented that partisans generally refrain from holding their own party elec-

torally accountable for economic outcomes (Bisgaard, 2015; Kayser and Wlezien, 2011; Rudolph, 2006), attributing any poor performance to some other factor than the competence of their preferred party (Tilley and Hobolt, 2011). Coalition governments are typically larger, electorally speaking, than single party governments. Accordingly, there will probably also be more voters who feel attached to a government party in a coalition government. When comparing the level of electoral accountability for single and multi-party governments, one may therefore be picking up the effect of differences in the number of government partisans rather than differences in the levels of incumbent responsibility.

These examples are not exhaustive in the sense that they cover all institutions which have been used to index how politically responsible the incumbent is for policy outcomes. Even so, these examples hopefully illustrate how the existing literature is challenged when it comes to identifying the causal effect of political responsibility on electoral accountability.

Adaptiveness of Accountability

Another interesting feature of the relationship between responsibility and accountability is how adaptive it is. That is, whether voters respond swiftly to short-term changes in political responsibility, continually adjusting how accountable incumbents are held for various outcomes, or whether this adjustment process works more slowly.

Previous studies have not paid much attention to the question of adaptiveness, mainly focusing on differences in incumbent responsibility which rarely change or which change slowly and incrementally (e.g., Duch and Stevenson, 2008; Ebeid and Rodden, 2006). This makes it hard to know how voters respond to sudden shifts in political responsibility. Some studies do examine more dynamic aspects of political responsibility, focusing on institutions which allocate different degrees of responsibility for policy outcomes to incumbent politicians over time within the same political unit (e.g., Carlin and Singh, 2015; Nadeau, Niemi and Yoshinaka, 2002). By focusing on this type of time-sensitive variation in the assignment of political responsibility, such studies could potentially tell us something about how adaptive the relationship between responsibility and accountability is. Yet these studies have rarely leveraged the dynamic nature of these institutions when examining how they affect electoral accountability. Instead they pool, either completely or partially, the within and between unit variation, making it impossible to get at whether the time-

sensitive (within unit) variation in political responsibility correlates with the extent to which voters hold incumbents accountable.

Adaptiveness is potentially quite important, because if voters are not adaptive, they risk holding their incumbent accountable for the quality of some outcome the incumbent is no longer responsible for – or they risk failing to hold the incumbent to account for the quality of an outcome the incumbent has recently become responsible for. Also, a lack of adaptiveness can give incumbents an incentive to neglect policy areas where they have recently become more politically responsible, because incumbents know that they will not be held accountable for their performance in these areas.

Policy-specificity

The existing literature has primarily examined the relationship between responsibility and accountability in terms of the economic and political institutions which affect how responsible incumbents are for a large set of policy outcomes. For instance, constitutional design (Anderson, 2006; Carlin and Singh, 2015), which broadly shapes incumbents' ability to affect economic and social outcomes, or different parliamentary practices (Nadeau, Niemi and Yoshinaka, 2002; Powell and Whitten, 1993; Whitten and Palmer, 1999), which shape incumbents' executive and legislative discretion across all policy areas. This focus on responsibility for a diverse and not clearly demarcated set of outcomes has made it difficult to assess how policy-specific voters are when they hold incumbents electorally accountable. In particular, we do not know whether voters link responsibility to accountability at the level of individual policy outcomes, weighing each outcome according to how responsible the government is for that specific outcome, or whether voters link responsibility and accountability at a more aggregate level, using different policy outcomes to form an overall evaluation of how their polity is doing, and then weigh this overall evaluation based on how responsible the incumbent is for policy outcomes in general.²

The previous literature cannot discriminate between a policy-specific and a more general relationship, because it looks at differences in incumbent responsibility for a diffuse set of policy outcomes (although for important exceptions, see Arceneaux,

²It is not theoretically straightforward to predict which of these approaches voters will adopt. On the one hand, adopting a policy-specific strategy seems to be more rational if one simply wants to learn more about the incumbent's competence (for evidence of this, see the appendix of Achen and Bartels, 2016). On the other hand, voters are often interested in employing heuristics and mental shortcuts (Downs, 1957; Kuklinski, Quirk et al., 2000). One such mental shortcut might be to link responsibility and accountability at an aggregate rather than a policy-specific level.

2006; Ruder et al., 2014). If one wanted to make inferences about policy-specificity, then one would need to examine a difference in political responsibility which only covered a discrete set of policy outcomes. In this case, it would be possible to examine policy-specificity by investigating whether voters *only* differed in how electorally accountable they held the incumbent for the policy outcomes for which there was an underlying difference in political responsibility, or whether electoral accountability for other outcomes was affected as well.

The Contribution of this Study

By focusing on a reform which changed political responsibility for a specific policy, the present study enables us to get at whether the relationship between responsibility and accountability is *policy-specific*. That is, we can examine whether voters only hold local incumbents more electorally accountable for the policy outcome affected by the reform, or whether voters hold incumbents more electorally accountable for other policy outcomes as well. Further, because we examine the effect of the reform at the first election after its implementation, any effect that we do find will reflect a relationship between responsibility and accountability which is reasonably *adaptive*. Finally, as discussed in more detail below, the assignment of the implementation of the reform which changed political responsibility was arguably exogenous, making it possible to identify the *causal* effect of the reform of electoral accountability.

Analyzing the extent to which the relationship between responsibility and accountability is causal, adaptive and policy-specific is important, because it tells us something about how adept voters are at electing competent politicians. In particular, all these factors make it more likely that voters only hold incumbents electorally accountable for outcomes the incumbent had a hand in shaping, which should, in turn, make it easier for voters to identify whether incumbent politicians have their best interests in mind, and reward them with reelection if they do (cf. Achen and Bartels, 2016; Anderson, 2006; Ashworth, 2012; Duch and Stevenson, 2008).³

³Too see this, note that if the relationship between responsibility and accountability is causal, then voters respond to changes in responsibility by holding incumbents more electorally accountable. If the relationship is policy-specific, then voters are more likely to shift their attention away from policy outcomes which incumbents have little responsibility for, and towards outcomes incumbents have more responsibility for. If the relationship is adaptive, then voters are more likely to act on the current distribution of political responsibility when holding incumbents accountable.

Research Design: Reform of Labor Market Regulation

In 2006, the administrative boundaries of Denmark were fundamentally redrawn, both in terms of geography, as 271 municipalities became 98, but also in terms of policy responsibilities, as the municipalities gained new responsibilities and lost others. Unfortunately, from a research standpoint, most of this extensive reform was implemented in all municipalities at the same time, making it hard to test how it affected the municipalities (although aspects of the reform have been leveraged in other contexts: see Bhatti and Hansen, 2011; Blom-Hansen, Houlberg and Serritzlew, 2014; Lassen and Serritzlew, 2011). One part of the reform, however, was not implemented at once, but in two steps: a reform of labor market regulation, which transferred the political responsibility for unemployment services from various agencies to municipal mayors.

Unemployment services constitute an important part of public service provision in Denmark. As such, Danish labor market policy has long been premised on the idea that the day-to-day interaction with the unemployed individual is important for reducing structural unemployment (Torfing, 1999). This idea is mirrored in spending priorities. According to the OECD, expenditures towards unemployment services (i.e., active labor market policies) represented 1.82 percent of the Danish GDP in 2013 compared to just 0.23 percent in the United Kingdom (OECD, 2016).

The labor market reform was first implemented in 14 municipalities (out of 98) in the beginning of 2007 and then implemented fully in 2010 (Ministry of Employment 2010), see figure 6.1 for a timeline.⁴

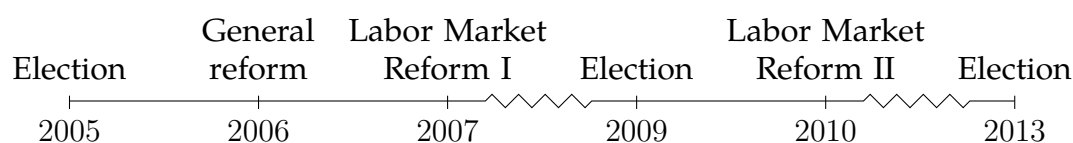


Figure 6.1 Labor Market Reform timeline. Labor Market Reform I was in the 14 “treatment” municipalities, Labor Market Reform II was the remaining “control” municipalities.

This reform provides a unique opportunity to investigate how voters react when incumbents get more political responsibility. As such, we can use the municipal elec-

⁴The municipalities were: Odense, Esbjerg, Vejle, Næstved, Herning, Sønderborg, Holbæk, Helsingør, Skanderborg, Høje-Taastrup, Thisted, Brønderslev-Dronninglund, Rudersdal and Rebild (Order 1400, 2006).

tions that took place in 2009 to compare the beliefs and behavior of the voters in the 14 municipalities where the mayor got more responsibility for unemployment services before the election – the treatment municipalities – with the beliefs and behavior of the voters in the 84 municipalities where the mayor did not get more responsibility until 2010 – the control municipalities. If the relationship between political responsibility and electoral accountability is causal, adaptive and policy-specific, then voters in the treatment municipalities should hold their mayor more electorally accountable for unemployment services than voters in the control municipalities.

To draw such inferences, however, one needs to make two assumptions about the reform. One is about the nature of the reform; that the reform exclusively affected municipal mayors' political responsibility for unemployment services. The other is about the assignment of the municipalities to early implementation of the reform (i.e., assignment to treatment); that assignment was independent of existing and potential levels of electoral accountability.⁵ Below, I explain why it is reasonable to make these assumptions about the reform, and then I present the data used to study the effect of the reform.

The Reform Only Affected Responsibility for Unemployment Services

The reform of labor market regulation made municipalities politically responsible for helping so-called 'insured workers' get back to work if they lost their job (i.e., the three-fourths of all workers who were members of an unemployment insurance fund). Before the reform was implemented, the national government and the unemployment insurance funds were responsible for the insured workers, whereas the municipalities were responsible for uninsured workers. The reform removed the unemployment insurance funds and the national government from the equation, and gave each municipality unilateral responsibility for all those who were out of a job in that municipality (Order 1400, 2006; Eskelinen 2008). After the reform, the municipalities were solely responsible for helping employers look for employees, finding and financing retraining for all unemployed workers, assisting unemployed workers with special needs, and for doing all this while maintaining efficient employment agencies.

⁵These assumptions roughly correspond to the exclusion and independence (or exogeneity) assumptions laid out by Dunning (2012) and Gerber and Green (2012). Along with the assumption of non-interference between units, they constitute the central assumptions needed to draw causal inferences. We do not discuss the non-interference assumption in detail, because political responsibility could not spillover to neighboring municipalities.

It is important to note that the reform did not simply increase local politicians' 'functional responsibility' for unemployment services (i.e., sense of obligation for unemployment services), but also their 'causal responsibility' (i.e., opportunity to affect the quality of unemployment services) (for details on these concepts, see Arceneaux, 2006, 735). Put differently, after the reform, the municipalities had more power – in the form of policy discretion and resources – to shape unemployment services for the better or for the worse.

While the reform ostensibly had an effect on who was politically responsible for unemployment services, reforms tend to be messy and have a very diverse set of long and short term consequences. In light of this, one might suspect that the assignment to the implementation of the labor market reform had important side-effects that could pose threats to the inferences I want to make below. However, if one examines the nature of the reform in more detail, such potential side-effects are not forthcoming. For one, the reform presents a very clean change in political responsibility for a specific policy: no responsibilities outside the area of unemployment services were conferred and no alternative regulation was implemented as part of the reform (Ministry of Employment 2010; Act 483, 2009).⁶

The Change in Responsibility was Exogenous

If one wants to draw causal inferences based on the selection of some municipalities for the early implementation of the labor market reform, then this selection process should be independent of existing and potential levels of electoral accountability (Gerber and Green, 2012). If it is not, one risks confounding the effect of the reform with the effect of being the type of municipality which is assigned to early implementation. Several factors surrounding the assignment of municipalities to early implementer status makes it likely that it was independent of existing and potential levels of electoral accountability.

First, the selection process was confined to municipalities within a single country at a single point in time. This makes it possible to rule out a host of possible confounders, such as macro-social developments and country-specific factors like culture and history.

Second, the final decision about which municipalities were assigned to implement the reform early was made by the central government rather than the municipalities themselves. In particular, employees at the ministry as well as the minister prepared

⁶See section S1 of the supplementary materials for some additional evidence of the fact that the reform did not have any important side-effects.

a list of municipalities which was then approved by the parties that voted for the reform in Parliament (Ministry of Employment, 2006).

Third, and most importantly, it seems likely that the ministry's assignment of municipalities to early implementation was independent of the municipalities' existing or potential levels of electoral accountability for unemployment services. For one, it is not clear that the ministry would have known what the level of electoral accountability for unemployment services was in the individual municipalities. Even if the ministry knew the levels, it is not clear that the team would have had an incentive to assign municipalities to early implementation based on these levels. There could feasibly have been an incentive to pick municipalities which generally fared better when it came to handling unemployment services, because these were more likely to make the reform look like a success (although I do not find any such imbalances between early and late implementers, cf. table 6.1), but it is unclear why the ministry should be interested in implementing the reform in places where the level of electoral accountability for unemployment services was particularly high (or low). Finally, even if the ministry did know and, for some reason, favored types of municipalities which had higher levels of electoral accountability, there were political forces at work which, arguably, muted any political favoritism.

When the reform was being negotiated, several actors were highly critical of giving the municipalities responsibility for unemployment services. As such, both the large unions and employer organizations, as well as the minority government's usual ally, in parliament, the Danish People's Party, were doubtful that the municipalities were up to the task (Kristensen, 2008, 88). Accordingly, there was pressure on the Ministry of Employment not to 'cherry-pick' municipalities based on past performance. As a person close to the selection process expressed it: "We were allowed to send up a test-balloon, but it was extremely important that they [the municipalities] were balanced."⁷ This sentiment is also mirrored in a press statement published by the Ministry of Employment, explaining how the 14 municipalities had been selected. In the statement, the Minister of Employment was quoted as saying that the goal had been to select "big as well as small municipalities, in cities as well as in rural areas". More generally, the Minister said "that the goal was to spread them out across the country" (Ministry of Employment 2006, author's translation). As such, specific types of municipalities were not targeted in the selection process. This is confirmed by looking at how the chairman of the organization Local Government

⁷Interview with Jan Handeliowitz, former employee at the Ministry of Employment. Author's translation.

Denmark, an organization representing the municipalities, reacted to the selection process. He said that the ministry's decision insured that "a broad cross section of municipalities [are represented], both size-wise and geographically." (Ritzau, 2006, author's translation). Additional evidence suggesting that the selection process was not politically motivated can be found if one looks at the reaction to the ministry's decision among those who were very critical of the reform: the unions and the employer organizations. As far as I have been able to determine, none of these political organizations officially criticized the government for having selected a biased or problematic set of municipalities for early implementation.⁸

Taken together, these factors suggest that the selection of the 14 early implementing municipalities was independent of existing and potential patterns of accountability. That is, based on the evidence presented here, there is reason to believe that the change in political responsibility for unemployment services was exogenous. This assertion is revisited below, where we show that the municipalities were balanced on important covariates.

Data and Measuring Electoral Accountability

To analyze the electoral consequences of the reform, I use the Danish municipal election survey (Elklit and Kjær, 2013). The 2009 election survey is of special interest, since this is where electorates in the treatment and control municipalities were governed by mayors with different levels of responsibility for unemployment services (cf. figure 6.1). Even so, the 2013 and 2005 surveys are used as well to test whether the electorates of the treatment and control municipalities differed before treatment (2005), and after all municipalities were treated (2013). Respondents in the municipal election surveys were recruited within six weeks of the municipal election using stratified random sampling in order to ensure that at least 30 respondents in each of the 98 Danish municipalities participated in the survey. The surveys are conducted partly via a web-survey and partly over the phone (for details about the surveys, see Elklit and Kjær, 2013).⁹

To measure the extent to which the mayor was held electorally accountable for unemployment services, I examine the correlation between voters evaluation of unemployment services and their propensity to support the municipal mayor (a

⁸This conclusion is based on an examination of all newspaper stories mentioning the reform in the month following the announcement of the assignment of municipalities to early-implementer status in the three major Danish broadsheets (Jyllands Posten, Politiken and Berlingske).

⁹The 2005 survey differs in this respect as it is not stratified according to municipality.

typical measure of electoral accountability, cf. Carlin and Singh, 2015; Duch and Stevenson, 2008; Stevenson and Duch, 2013), interpreting a higher correlation as evidence that the mayor is being held more electorally accountable for the quality of unemployment services. To assess voters' evaluation of unemployment services, the following survey item is used: "How satisfied or unsatisfied are you in general with the municipality's efforts towards the unemployed?" Answers were recorded on a five-point Likert scale going from "very dissatisfied" to "very satisfied".¹⁰ To measure support for the incumbent mayor, I look at whether respondents reported voting for the incumbent mayor's party at the municipal election. Respondents who did not vote and respondents who could not remember which party they voted for are omitted from the analysis.¹¹

This measure of electoral accountability is not perfect, and will probably contain some measurement error. In particular, the measure might also capture, at least in part, the extent to which voters form beliefs about unemployment services based on who they vote for (so-called motivated reasoning, cf. Tilley and Hobolt, 2011). Accordingly, this measure might overestimate the *level* of electoral accountability in each municipality. Even so, we will still be able to get an unbiased estimate of the *difference* between treatment and control, as long as this measurement error is not correlated with treatment status (King, Keohane and Verba, 1994, chap. 5).¹²

All survey items used in the analysis are described in section S2 of the supplementary materials, and descriptive statistics on all variables can be found in section S3 of the supplementary materials.

Analysis

The goal of this analysis is to find out whether voters in the treatment municipalities held their mayor more electorally accountable for the quality of unemployment services than voters in the control municipalities. The analysis will also explore

¹⁰The survey-item on unemployment services was not included in the 2013 survey. The large reform in 2005 meant that a majority of municipalities had been amalgamated, and therefore most voters did have an incumbent mayor in the 2005 election. Therefore, I can only effectively measure electoral accountability for unemployment services in the 2009 election.

¹¹Support for the mayoral party is used to measure support for the mayor, because voters do not elect mayors directly in Denmark. Rather, they elect members of a city council, and the city council then appoints a mayor right after the election (Houlberg and Pedersen, 2015). Municipal elections in Denmark are held every four years in November. The electoral system is proportional representation and in most municipalities there is a multi-party system which mirrors the national party system.

¹²In the section 'Alternative Explanations', this assumption is discussed further and tested empirically (see also section S8 of the supplementary materials).

whether mayors in treatment municipalities were held more electorally accountable for the quality of other services as well. In addition to this, I examine the viability of some alternative explanations. Before these analyses are presented, however, a balance-test and a manipulation check is laid out in order to investigate whether assignment to treatment (ie., early implementation of the reform) was exogenous to electoral accountability, and whether being assigned to treatment had an impact on voters' beliefs about the distribution of political responsibility.

Balance Test and Manipulation Check

Table 6.1 compares treatment and control municipalities, before they were treated, on a number of individual-level and municipality-level variables. For the individual-level variables there are no statistically meaningful differences. Most importantly, there are no differences across treatment and control on several variables that are likely to be highly correlated with electoral accountability, such as knowledge about municipal powers and interest in local politics (Vries and Giger, 2014). This is consistent with the qualitative evidence, laid out above, which suggested that implementation of the reform was assigned to municipalities independently of existing levels of electoral accountability. If particular types of electorates had a higher probability of being assigned to early implementation, it seems likely that one would be able to identify systematic differences across treatment and control municipalities, but no such differences are identified.

The municipal level variables paint roughly the same picture. Across the different variables, only one shows a significant difference between the two groups – the treatment municipalities had a slightly larger number of inhabitants than the control municipalities. Even so, examining the standardized differences for the remaining municipal level variables, there does seem to be some substantial, though statistically insignificant, differences across treatment and control. This is not that surprising. The number of observations at the municipal level is relatively low, which means that the random variation between treatment and control could be relatively high. Nonetheless, these random imbalances might skew the results one way or another. When analyzing the differences between the treatment and control municipalities below, I take this issue into account by controlling for the municipal level variables which have the largest standardized differences (i.e., proportion of national government voters/mayors, female office seekers, non-Western citizens and number of inhabitants).

Table 6.1 Were the treatment and control municipalities different?

Variable	Treatment	Control	Std. dif.	p-value	n
<i>Individual-level variables (2005)</i>					
Informed	0.44	0.45	-0.07	0.16	1996
Interested	0.63	0.64	-0.06	0.31	1884
Unemployment performance	0.47	0.48	-0.03	0.68	1454
Knowledge about municipal powers	0.70	0.71	-0.05	0.37	2011
Elderly performance	0.64	0.67	-0.09	0.15	1534
Housing performance	0.74	0.75	-0.05	0.40	1944
Ideology	0.69	0.70	-0.00	0.97	2011
Apathy	0.14	0.15	-0.00	0.93	1988
Obligation	0.95	0.96	-0.07	0.25	2000
Satisfaction with municipal democracy	0.52	0.52	-0.02	0.71	1975
Pivotality	0.45	0.46	-0.00	0.95	1875
<i>Municipality-level variables (2006)</i>					
Population density (log)	2.18	2.25	-0.12	0.60	98
More than high-school educ. (pct.)	23.10	21.54	0.19	0.51	98
Unemployment rate (pct.)	2.16	2.31	-0.14	0.48	98
Citizens with non-Western origins (log)	2.39	2.32	0.27	0.27	98
Female municipal office-seekers (pct.)	28.13	30.39	-0.46	0.13	98
Municipal tax rate (pct.)	24.55	24.77	-0.23	0.31	98
Social transfers (log)	4.13	4.13	0.03	0.91	98
Services contracted out (0-100 scale)	21.90	22.78	-0.22	0.35	98
Spending on active labor market (log)	3.21	3.20	0.06	0.77	98
Inhabitants (log)	4.82	4.58	0.69	0.00	98
National government voters	0.42	0.38	0.31	0.17	98
National government mayor	0.64	0.45	0.38	0.20	98

Individual-level variables from the 2005 municipal election survey, see Appendix A for detailed description. Municipality-level variables taken from Statistics Denmark. p-values from difference in means test. National government voters is the proportion of voters who voted for parties in government at the municipal election in 2005. Standardized difference computed as difference in means divided by standard deviation in the control group. Heavily skewed variables presented on a logarithmic scale.

Another relevant issue is whether voters in the treated municipalities actually updated their beliefs about the mayor's responsibility for unemployment services. That is, whether the reform actually registered with the voters. Unfortunately, there is no question in the municipal election survey that directly probes voters' beliefs about the extent of their mayor's responsibility for unemployment services. However, there are two questions which ask respondents about how much political responsibility local politicians have for conditions in the municipality in general. The first of these questions ask voters whether the mayor and other local officials (rather than national politicians) has the primary responsibility for how the municipality developed in the last four years. The second question asks voters about the extent to which the mayor has had an effect on the well-being of the municipality. Analyzing voters' responses to these questions, I find that the voters in the treatment municipalities believed that their local politicians were more responsible for and had greater a influence on conditions in their municipality ($p < 0.05$; see section S4 of the supplementary materials for details).

Electoral Accountability for Unemployment Services

Figure 6.2 plots the conditional probability of supporting the mayoral party in the 2009 election across voters' satisfaction with unemployment services in the treatment and in the control municipalities. The figure also plots a linear fit of the relationship between voters' satisfaction with unemployment services and support for the mayoral party. This graphical analysis allows us to compare the extent to which voters' evaluations of unemployment services shape incumbent support in the treatment versus the control municipalities.

The figure shows that support for the mayoral party was more closely related to voters' evaluation of unemployment services in the treatment municipalities. Accordingly, the increase in local political responsibility for unemployment services seems to be associated with an increase in the extent to which voters punished and rewarded local incumbents for the quality of these services.

To investigate further, I estimate a regression model which sets the probability that the respondent voted for the mayoral party as a logistic function of the respondent's evaluation of the municipality's performance in the area of unemployment services, an indicator variable determining whether the respondent lived in a treatment or a control municipality, as well as an interaction between the two. The model also includes a small number of control variables: voters' satisfaction with elderly care service, and their satisfaction with housing management, as well as their beliefs

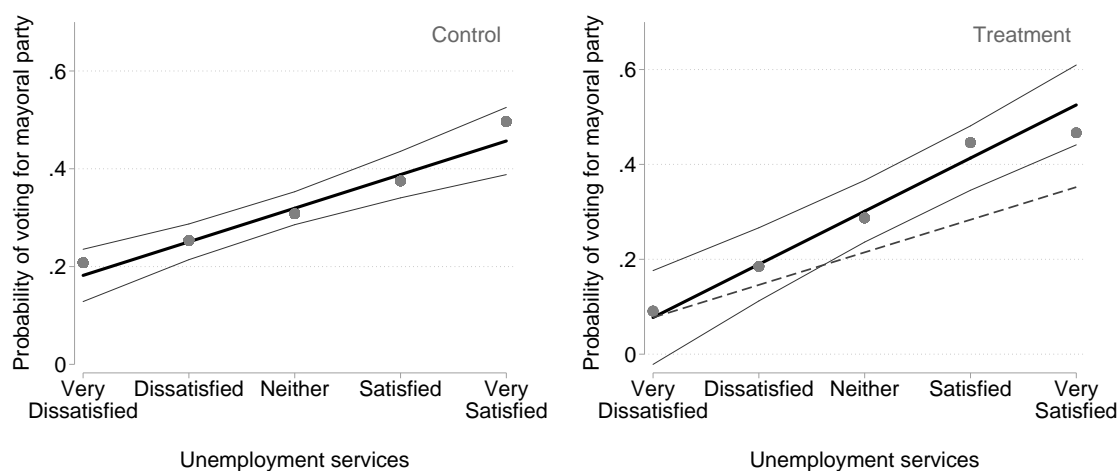


Figure 6.2 Solid lines reflect the predicted probability of voting for the mayoral party across satisfaction with unemployment services in treatment and control municipalities with 95 pct. confidence intervals. The predictions are derived from a linear probability model with a treatment by unemployment services interaction. Standard errors are clustered at the municipal level. Interaction estimate is statistically significantly different from zero ($t(1521) = 2.08, p = 0.04$). Dots are conditional probabilities estimated from the sample ($n = 1,522$). The dashed line in the “Treatment” plot reflects the slope from the “Control” plot.

about how powerful the municipal administration is.¹³ These variables are meant to reduce the error term of the model, and control for trends in performance and beliefs about how responsible local politicians are for economic and social outcomes. Since the treatment was assigned to municipalities, standard errors are clustered at the municipal level when estimating the model. The estimates from this model are presented in column one of table 6.2.

The primary estimate of interest is the interaction between treatment and unemployment performance. This coefficient indicates whether voters’ evaluation of the quality of unemployment services was more or less closely tied to the propensity to vote for the mayoral party in the treatment municipalities. The coefficient is statistically significant ($p < 0.05$) and positive. This means that satisfaction with the municipality’s unemployment services mattered more in the municipalities where the mayor had more political responsibility for unemployment services. The remaining coefficients in the model have the expected sign, and, apart from housing performance, are all statistically significant.

¹³I also estimated a more simple logistic model, without any controls. The interaction estimate in this simple model is also statistically significant, and of roughly the same size as the one presented in column one of table 6.2.

A similar picture emerges if one derives the average marginal effects (AMEs) of satisfaction with unemployment services for the treatment and control municipalities (for a description of the statistical properties of AMEs, see Hanmer and Ozan Kalkan, 2013). The AMEs are reported at the bottom of column one in table 6.2, and they reveal that in the control municipalities, the result of going from one end of the unemployment scale to the other end is an average increase in the probability of voting for the mayoral party of 17 percentage points. In the treatment group, the result is an increase of 36 percentage points, a difference which is statistically significant ($p < 0.05$) and quite large: the AME for voters in the treatment municipalities is twice that for voters in the control municipalities.

Table 6.2 Logistic regression of probability of voting for the mayoral party

	(1)	(2)	(3)	(4)
Unemployment performance	0.82* (0.23)	0.83* (0.23)	0.94* (0.36)	0.85* (0.35)
Treatment	-0.61 (0.39)	-0.59 (0.40)	-0.52 (0.43)	-0.63 (0.41)
Treatment * Unemployment performance	1.00* (0.51)	0.98+ (0.52)	1.25* (0.62)	1.33* (0.58)
Administration controls municipality	-0.36+ (0.21)	-0.46* (0.21)	-0.48* (0.24)	-0.43+ (0.24)
Elderly performance	0.76* (0.23)	0.81* (0.25)	1.03* (0.33)	1.03* (0.30)
Housing performance	0.38 (0.29)	0.31 (0.30)	-0.13 (0.38)	0.04 (0.39)
Sociodemographic controls		✓	✓	✓
Political controls			✓	✓
Municipal level variables				✓
AME (Control)	0.17 (0.05)	0.17 (0.05)	0.10 (0.04)	0.09 (0.04)
AME (Treatment)	0.36 (0.08)	0.35 (0.08)	0.26 (0.06)	0.25 (0.05)
Difference (T-C)	0.19	0.18	0.15	0.15
<i>p-value of difference</i>	0.03	0.04	0.04	0.01
Pseudo R ²	0.04	0.05	0.41	0.42
Log likelihood	-921	-894	-561	-552
Observations	1522	1500	1500	1500

Robust standard errors clustered on municipality in parentheses.

+ $p < 0.10$, * $p < 0.05$

These analyses indicate that there is a causal relationship between political responsibility and electoral accountability. In the municipalities where the mayor was exogenously assigned more responsibility for unemployment services, the mayor's party was also held more electorally accountable for the quality of these services. The analyses also imply that there is quite an adaptive relationship between responsi-

bility and accountability. The reform which affected how responsible the municipal mayor was for unemployment services was implemented just three years prior to the election analyzed above. In spite this, voters did respond to the change, holding their mayor more electorally accountable.

Below, I show that these results are quite robust. In particular, I add more controls to the regression model estimated above, then I try out some alternative estimation techniques, and finally, I re-analyze the effects of the reform using a difference-in-difference approach.

Additional controls: First, a number of socio-demographic variables are included in the regression model (i.e., age, gender education, occupational status, and local media consumption). The controls are included in the model estimated in the second column of table 6.2. Introducing these controls only shifts the estimates slightly, and the difference in AMEs between the treatment and control municipalities remains statistically significant ($p < 0.05$). Second, a set of political variables are added to the model. To gauge partisanship, I include a control for whether the respondent would vote for the mayor's party if a *national* election was held tomorrow, and a variable indicating whether the respondent shares the ideological orientation of the mayor. I also include a variable indicating whether the mayor was from the same party as one of the governing parties at the national level, something which might make the mayor susceptible to blame for unemployment services (cf. Cutler, 2008). These political controls are included in the third column of table 6.2. Introducing the political controls reduces the *overall* AME of unemployment performance, but does not substantially reduce the *difference* between the treatment and control municipalities, which remains statistically significant. Finally, a battery of municipal level control variables are included. The municipal level variables included are the ones for which the balance test revealed a substantial imbalance (cf. above). These controls are included in column four of table 6.2. The interaction effect and difference in AMEs remain statistically significant in this specification as well.

Alternative Estimation Methods: Section S5 of the supplementary materials examines whether the results are sensitive to alternative ways of estimating the interaction effect and its sampling variability. Specifically, I use a multi-level logit model and a form of randomization inference. Using these alternative estimation methods, the difference in the AME of unemployment performance between treatment and control municipalities remains statistically significant ($p < .05$ for multi-level models, $p < 0.1$ for randomization inference).

A Difference in difference approach: Section S6 of the supplementary materials examines whether similar results can be obtained using a difference-in-difference approach. Here, data from the 2005 and 2009 municipal election surveys are pooled in order to estimate a model which examines whether the difference in the effect of unemployment services between treatment and control municipalities is different across the two elections. This controls for any pre-treatment differences there might have been in levels of electoral accountability for unemployment services. This analysis reveals that there are no apparent differences across treatment and control municipalities in 2005, and that the difference identified in 2009 is statistically distinguishable from the difference in 2005 ($p < 0.1$). In sum, the difference-in-difference analysis reaffirms the findings from the more simple analysis presented in table 6.2. It is important to note, however, that due to the large municipal reform in 2006 the 2005 and 2009 data are not perfectly comparable, making the cross-sectional 2009 analysis my preferred test of whether the reform affected electoral accountability for unemployment services (see the supplementary materials for details).

Electoral accountability for other outcomes

If voters link responsibility and accountability at a general level, rather than at the level of each specific policy, then changes in political responsibility for one policy outcome should lead voters to hold elected officials more electorally accountable for other policy outcomes as well. To investigate whether this was the case in this context, I look at how electorally accountable voters in the treatment and control municipalities held their mayor for the quality of two types of public services *not* affected by the labor market reform: housing and elderly care. These two types of services are privileged because they represent a key policy that local governments typically deal with, housing, and a service which shares several features of the unemployment services examined above, elderly care.¹⁴ In addition to this, I look at ideological congruence with the mayoral party to see if voters are more electorally responsive to their mayor's ideological orientation in the treatment municipalities.

To examine electoral accountability for these alternative policies, an extended version the model presented in the fourth column of table 6.2 is estimated. This model adds interactions between the treatment indicator and voters' satisfaction

¹⁴In particular, elderly care only directly affects a certain target population (i.e., the elderly), similarly to how unemployment services only affect the unemployed. Elderly care is also similar to unemployment services in that it is a public service which consists of direct contact with municipal employees.

with their municipality's housing management, their municipality's elderly care and voters' ideological congruence with their municipality's mayor. Figure 6.3 graphs the key estimates derived from this extended model – the AMEs of the alternative policy variables across voters living in the treatment and control municipalities. For comparison, the AMEs of unemployment performance are also plotted.

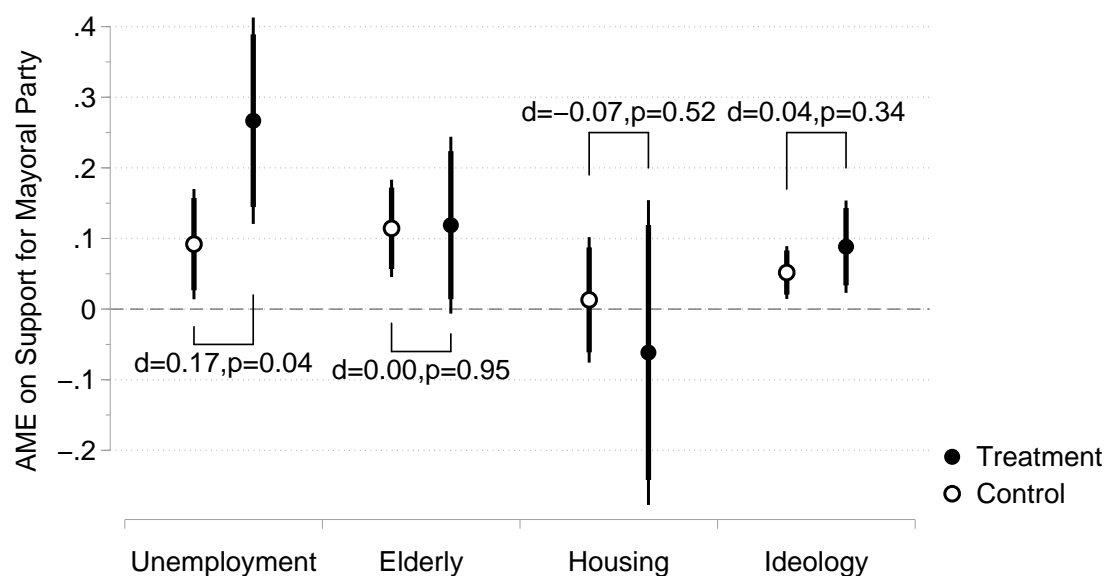


Figure 6.3 Average marginal effects of different variables on probability of voting for the mayoral party in treatment and control municipalities. All average marginal effects derived from an augmented version the model presented in the fourth column of table 6.2 which includes interactions between each variable and the treatment indicator; McFadden $R^2 = 0.41$, $n = 1,500$. Wald tests used to compare the different AMEs. The vertical lines are 90 pct. (thick) and 95 pct. (thin) confidence intervals.

As can be seen from figure 6.3, there are no statistically discernible differences across treatment and control municipalities for the AMEs of elderly care, housing and ideology. This is in contrast to unemployment services, where there is a substantial and statistically significant difference.¹⁵

In sum, there is no sign that voters held their mayor more electorally accountable for other policy outcomes than the one for which additional political responsibility

¹⁵Section S7 of the supplementary materials analyzes the robustness of these results by running similar analyses for a number of other policy areas. The results of these analyses are laid out in section S7 of the supplementary materials. Among the seven additional policies examined, there is not a single statistically significant difference between the treatment and control municipalities.

was conferred, suggesting that the relationship between responsibility and accountability was policy specific.

In addition to policy-specificity, these findings also speak to the question of whether the relationship between responsibility and accountability for unemployment services identified in table 6.2 is causal. If there were differences across treatment and control municipalities in how accountable voters held their mayor for some other type of performance, then this might have been because the electorates in the treatment municipalities were more likely to attribute political responsibility for all types of outcomes to the mayoral party in the 2009 election – irrespective of any objective differences in political responsibility. In this sense, the analysis of the other policy areas can also be interpreted as a placebo test.

Alternative explanations: Political attention and Priming

When a policy area is subject to increased political subjugation it seems natural that it will become more politically contested. Accordingly, increased local political responsibility for unemployment services in a municipality could have lead to increased local political attention to this issue in the campaign, priming the issue in the minds of the voter (Krosnick and Kinder, 1990). Based on this, one might wonder: is the change in electoral accountability for unemployment services really based on the fact that this issue was “primed” in the treatment municipalities?

This might be the case. More generally, priming may help explain why political responsibility is related to electoral accountability: responsibility leads to attention and attention leads to accountability. Some evidence suggests that priming can play such a role (Hart, 2016; Ruder et al., 2014). It is important to note, however, that the present investigation does not hinge upon figuring out whether this is the case. This study remains agnostic about the mechanism underlying the relationship between political responsibility and electoral accountability, and the reform studied here is not well suited for figuring out whether priming is what is driving the relationship. Instead, the focus of this study has been on whether the responsibility-accountability relationship can be characterized as causal, adaptive and policy-specific. Even so, if we accept that priming is the mechanism in the present context, which it might be, then the increased attention to unemployment services, which any priming would presuppose, might have other side-effects that challenge the inferences made above.

For one, increased attention to unemployment services might have lead to increased polarization between mayoral-partisans and non-partisans in beliefs about unemployment services. In particular, if unemployment services were framed by

elite actors as a more important issue in the treatment municipalities, then mayoral partisans would also be more strongly motivated to engage in partisan rationalization when forming their beliefs about the quality of unemployment services (for evidence of such a mechanism see Parker-Stephen, 2013). If this is the case, then the increased correlation between voters' assessment of unemployment services and support for the incumbent mayor might not just be a result of voters holding the mayor more electorally accountable, but also of voters relying more on their partisan preconceptions (i.e., reverse causation from voting to beliefs about unemployment services might be stronger in the treatment municipalities). To explore the viability of this alternative explanation, section S8 of the supplementary materials examines whether beliefs about unemployment services are more correlated with *past* support for the mayor party in the treatment municipalities. There is no evidence of this. As such, support for the mayor at the last election does not seem to have a greater bearing on voters' evaluation of unemployment services in the treatment municipalities.

A related concern is that the increase in electoral responsibility is not simply the result of a change in political responsibility but the result of a *recent* change in political responsibility. That is, changes in the distribution of political responsibility might have short lived priming-effects, which moderate voters' attention to the policies for which responsibility is changed in the immediate aftermath of this change. The implication being that there was no permanent shift in how accountable the mayor was held for unemployment services, and accordingly no lasting relationship between responsibility and electoral accountability. This alternative explanation is hard to test in the present context, as we cannot look at the long term differences between the reformed (i.e., treated) and unreformed (i.e., control) municipalities. In the election following the one examined above, all municipalities had implemented the reform, which increased political responsibility for unemployment services. Even so, we do at least know that if the results obtained above are the consequence of priming, then this priming cannot have been *very* short-lived. After all, the election examined above occurred a few years after the reform was first implemented in the treatment municipalities, and while this is a short period of time compared to the rate at which other political institutions change (cf. the section on adaptiveness), it is not a short period of time compared to the news cycle of typical elections (Rosenberg and Feldman, 2008). In addition to this more theoretical consideration, there is one feasible empirical test of the short-lived priming explanation. This test is based on the 2013 municipal election. In the 2013 election, responsibility for unemployment services was constant across the treatment and control municipalities,

because all municipalities had implemented the labor market reform; however, the time since implementation varied. In the late-implementing control municipalities, the reform was relatively new (implemented three years before the election); in the early-implementing treatment municipalities the reform was not new (implemented seven years before the election). Section S9 in the supplementary materials examines whether this difference in time since implementation affected the extent to which unemployment services were on the voters' agenda in the 2013 election (i.e., whether the implementation of the reform primed voters to focus on unemployment services), but finds no evidence of such a short-lived priming effect.

In conclusion, there is no evidence to suggest that the side-effects of potential increases in political attention – such as increased partisan polarization or short-lived priming – can explain the local electorates' behavior in the wake of the labor market reform. This reaffirms the initial interpretation – namely, that the local electorates' behavior in the wake of the labor market reform can be explained by a relationship between political responsibility and electoral accountability which is causal, adaptive and policy-specific.

Conclusion

This article examined a reform of Danish labor market regulation in which some municipal mayors were assigned more political responsibility for a specific policy. The article found that in the election following this reform, voters held these mayors more electorally accountable for the outcomes of this policy. This is especially noteworthy because the increase in political responsibility was exogenous, because the increase happened just a few years prior to the election, and because the article also found that voters *did not* hold their mayor more electorally accountable for the outcomes of policies unaffected by the reform. As such, the findings suggest that when an incumbent's political responsibility for a specific policy changes, this has a practically immediate causal effect on how electorally accountable voters hold this incumbent for the outcomes of this specific policy. This empirical characterization of the relationship between political responsibility and electoral accountability considerably advances the literature on this topic, because we knew little about the extent to which the relationship was causal, adaptive or policy-specific prior to this study.

How generalizable is this characterization of the relationship between political responsibility and electoral accountability? The change in political responsibility examined in this study was abrupt and exogenous – this makes the case inherently

special. Nonetheless, there are reasons to believe the findings do generalize. Several features of the elections studied here are relatively common. For instance, this study focused on a multi-party system with proportional representation. Most countries have multi-party systems with proportional representation. Some might argue that Denmark is a likely case for identifying a relationship between political responsibility and electoral accountability, because of the relatively high levels of political knowledge and interest (Hansen and Pedersen, 2014). However, this potential threat to generalizability is arguably ameliorated by focusing on local elections, which are generally followed less closely (Elklit and Kjær, 2013). That is, a Danish voter in a local election might be more like an average voter in a national election when it comes to political engagement. Even so, more studies in other contexts investigating the extent to which the relationship between responsibility and accountability can be characterized as causal, adaptive and policy-specific are needed to pin down the exact scope of the article's findings.

Turning to implications, a more causal, adaptive and policy-specific relationship between political responsibility and electoral accountability should mean that voters tend to elect more competent politicians. As such, if voters are able to hold politicians more electorally accountable for the policy outcomes that the politicians are more responsible for, then it will also be more likely that voters will select politicians based on the quality of outcomes that accurately reflect these politicians' efforts and abilities (Achen and Bartels, 2016; Duch and Stevenson, 2008). Even so, a causal, adaptive and policy-specific relationship between responsibility and accountability is no panacea for effective democratic control. For one, as Ashworth (2012) and others have argued, voters may end up holding politicians accountable for policy outcomes which voters think are relatively unimportant, but which incumbent politicians are clearly responsible for (e.g., Ashworth, Bueno de Mesquita and Friedenberg, 2017). Nonetheless, the results suggest that to the extent that it is possible to sort competent politicians from incompetent ones, voters will try to do so by identifying how responsible politicians are for the state of specific policy outcomes, and electorally punish or reward them accordingly.

Supplementary materials

S1: The Broader Legislative Context of the Reform

Above I argued that the reform of labor market regulation changed how politically responsible municipalities were for unemployment services. A concern one might have with this argument, is that the national government somehow undid the effects of the reform by introducing detailed legislation instructing municipalities on how they should administer unemployment services, leaving the municipalities with no real administrative discretion. If this is the case, then implementing the reform would simply have meant trading a clear limit to the municipalities' political responsibility for an opaque limit. However, if one studies the reform legislation (Order 1400, 2006), there is no sign of any such detailed regulation instructing municipalities on how to administer unemployment services. Further, if one explores the amount of enacted national legislation related to labor market regulation around the implementation of the reform, one does not find any marked increase. On the contrary, an examination of the legislation coming from the Ministry of Employment between 2005 and 2011 reveals that, while additional statutes and laws were being instated, fewer were instated in this period than between 1998 and 2004 and between 1991 and 1997 (to examine this, I used data from Jakobsen and Mortensen, 2014). As such, I find no evidence suggesting that the national government tried to take back some or all of the political responsibility for unemployment services delegated to the municipalities as part of the labor market reform.

S2: Variable Descriptions

Table S.6.1 presents a short description of the different survey items used in the analysis.

Table S.6.1 Description of survey items from the municipal election surveys

Variable name	Question	Coding
Reelect mayor	‘Who did you vote for in the previous election?’	1 is for mayoral party voters, 0 is for the other party’s voters.
Treatment	‘What municipality do you live in?’	1 indicates 14 treatment municipalities, 0 the 84 control municipalities.
Unemployment performance	‘How satisfied or unsatisfied are you in general with your municipality’s efforts towards the unemployed?’	Five point scale going from 0 “Very unsatisfied” to 1 “Very satisfied”.
Elderly performance	‘How satisfied or unsatisfied are you in general with your municipality’s efforts towards the elderly?’	Five point scale going from 0 “Very unsatisfied” to 1 “Very satisfied”.
Housing performance	‘How satisfied or unsatisfied are you in general with your municipality’s efforts towards private and public housing?’	Five point scale going from 0 “Very unsatisfied” to 1 “Very satisfied”.
Administration controls municipality	‘In reality, the administration controls the municipality, not the politicians’	Five point scale going from 0 “Completely disagree” to 1 “Completely agree”.
Partisanship	‘Who would you vote for if a national election was held tomorrow?’	1 if respondent voted for mayoral party, 0 otherwise.
Mayoral party in government	Indicator variable of whether the mayor’s party is in government	1 for mayors from the two governing parties in 2009, 0 for other mayors.

Ideology	Measures congruence between respondent's ideology (left or right-wing) and the ideology of the mayor. Mayoral ideology determined based on party (Conservative and Liberal party as right wing), respondent's ideology based on question about self-reported ideology.	Coded 1 if respondent shares ideology with mayor, coded 0 if respondent does not.
News consumption - local	'Thinking back, how important was local media as a source of knowledge about the municipal election campaign?'	Four point scale going from 1 "Not at all" to 5 "Very important".
Age	'How old are you?'	Measured in years.
Employment status	'Where are you currently employed?'	11 different categories including student, unemployed and retiree.
Knowledge about municipal powers	Five different questions about who has responsibility for various policy areas.	Proportion of correct answers.
Interest	'How interested would you say you are in politics?'	Four point scale going from 0 "Not at all" to 1 "Very".
Informed	'How informed would you say you are about municipal politics in your own municipality?'	Five point scale going from 0 "Not at all informed" to 1 "Very informed".
Influence	'The mayor has a great deal of influence on how the municipality develops'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".
Responsible	'Who do you think has the main responsibility for things going as they have in the past four years in your municipality?'	Respondents answering "Local politicians" or "The Mayor" coded 1. Respondents answering "National politicians" coded 0.
Apathy	'I cannot be bothered with the municipal election'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".

Obligated	'I feel obligated to vote at the municipal election'	Five point scale going from 0 "Completely disagree" to 1 "Completely agree".
Satisfied with democracy	'How satisfied are you with the local democracy?'	Four point scale going from 0 "Not at all satisfied" to 1 "Very satisfied"
Pivotality	'How likely is it that your vote will be pivotal?'	Five point scale going from 0 "Basically zero" to 1 "Very probable"

S3: Descriptive statistics

Tables S.6.2, S.6.3 and S.6.4 present descriptive statistics on the survey items used in the analysis of the 2005, 2009 and 2013 municipal election surveys.

Figure S.6.1 presents the distribution of the key unemployment performance variable across treatment and control.

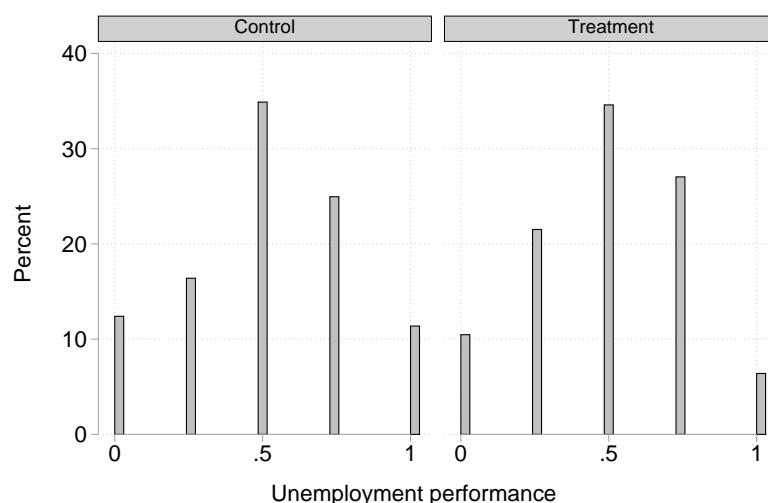


Figure S.6.1 Distribution of variable unemployment performance.

Table S.6.2 Descriptive statistics 2005

	Mean	SD	Min	Median	Max	n
Informed	0.45	0.19	0.00	0.40	1.00	1996
Interested	0.64	0.26	-0.33	0.67	1.00	1884
Unemployment performance	0.47	0.25	0.00	0.60	1.00	1454
Knowledge about municipal powers	0.71	0.28	0.00	0.80	1.00	2011
Elderly performance	0.66	0.31	0.00	0.75	1.00	1534
Housing performance	0.75	0.26	0.00	0.75	1.00	1944
Ideology	0.70	0.46	0.00	1.00	1.00	2011
Apathy	0.15	0.27	0.00	0.00	1.00	1988
Obligation	0.96	0.16	0.00	1.00	1.00	2000
Satisfaction with municipal democracy	0.52	0.17	0.00	0.50	1.00	1975
Pivotality	0.45	0.28	0.00	0.50	1.00	1875

Table S.6.3 Descriptive statistics 2009

	Mean	SD	Min	Median	Max	n
Vote for mayoral party	0.31	0.46	0.00	0.00	1.00	2742
Vote for mayorak party at national elections	0.23	0.42	0.00	0.00	1.00	3199
Voted for mayoral party at regional election	0.25	0.43	0.00	0.00	1.00	3199
Voted for mayoral party at last municipal election	0.30	0.46	0.00	0.00	1.00	3199
Voted for mayoral party at last national election	0.26	0.44	0.00	0.00	1.00	3199
Influence	0.74	0.25	0.00	0.75	1.00	3175
Responsibility	0.67	0.47	0.00	1.00	1.00	2998
Unemployment performance	0.51	0.29	0.00	0.50	1.00	2296
treatment	0.15	0.36	0.00	0.00	1.00	3336
Housing performance	0.71	0.23	0.25	0.75	1.00	2920
Elderly performance	0.57	0.31	0.00	0.50	1.00	2771
Administration controls municipality	0.50	0.31	0.00	0.50	1.00	2895
Local media consumption	3.14	0.92	1.00	3.00	5.00	3336
Age	54.53	13.79	18.00	55.50	91.00	3272
Shares ideology with mayoral party	0.44	0.50	0.00	0.00	1.00	3336
Mayor is from the same party as national government	0.49	0.50	0.00	0.00	1.00	3199
Elementary school	0.18	0.38	0.00	0.00	1.00	3336
High school	0.07	0.26	0.00	0.00	1.00	3336
Vocational high school	0.04	0.19	0.00	0.00	1.00	3336
Vocational school	0.25	0.44	0.00	0.00	1.00	3336
Shorter tertiary education	0.08	0.27	0.00	0.00	1.00	3336
Tertiary education	0.23	0.42	0.00	0.00	1.00	3336
Graduate degree	0.03	0.17	0.00	0.00	1.00	3336
Postgraduate degree	0.12	0.33	0.00	0.00	1.00	3336
Untrained worker	0.05	0.21	0.00	0.00	1.00	3336
Skilled worker	0.11	0.31	0.00	0.00	1.00	3336
Blue collar worker	0.13	0.34	0.00	0.00	1.00	3336
White collar worker	0.20	0.40	0.00	0.00	1.00	3336
Self employed	0.06	0.24	0.00	0.00	1.00	3336
Home maker	0.00	0.05	0.00	0.00	1.00	3336
Student	0.03	0.16	0.00	0.00	1.00	3336
Not looking for work	0.00	0.06	0.00	0.00	1.00	3336
Unemployed	0.04	0.19	0.00	0.00	1.00	3336
Retiree	0.34	0.47	0.00	0.00	1.00	3336
Will not say	0.01	0.10	0.00	0.00	1.00	3336
Other	0.03	0.18	0.00	0.00	1.00	3336

Table S.6.4 Descriptive statistics 2013

	Mean	SD	Min	Median	Max	n
Responsibility	0.78	0.41	0.00	1.00	1.00	3968
Influence	0.72	0.22	0.00	0.75	1.00	4254
Unemployment services: most important	0.13	0.34	0.00	0.00	1.00	3205
Unemployment services: should be most important	0.09	0.29	0.00	0.00	1.00	3205

S4: A Manipulation check

Mean responses for the two manipulation check questions are presented in the two left-most columns of table S.6.5. It is important to note that while the differences between treatment and control are not very large, these questions are about conditions in the municipality in general, not just unemployment services. While I would expect that voters in the treatment municipalities believe the mayor is substantially more responsible for unemployment services, I would only expect that voters believe the mayor is slightly more responsible for the overall conditions in the municipality.

Table S.6.5 Manipulation check

	2009		2013	
	Responsible	Influence	Responsible	Influence
Control	0.66 (0.01)	0.74 (0.01)	0.78 (0.01)	0.72 (0.00)
Treatment	0.71 (0.02)	0.76 (0.02)	0.78 (0.01)	0.72 (0.01)
p-value	0.03	0.03	0.39	0.25
Observations	2998	3175	3968	4254

Standard errors in parentheses, one-sided p-value from difference in means test.

Was the 2009 difference due to pre-treatment differences in voters' beliefs? This seems unlikely given the extensive balance-test presented in table 6.1, but we cannot be sure because these questions were not posed in the 2005 survey. However, these questions were part of the 2013 municipal election survey and we can utilize the 2013 data to conduct a post-treatment balance test. Recall that, when the 2013 election came about, the reform was implemented in all municipalities. As such, if the differences in the 2009 survey were due to the asymmetry in political responsibility caused by the reform, these differences should have disappeared in 2013. The two right-most columns of table S.6.5 report means across the treatment and control municipalities from the 2013 survey. As expected, once all of the municipalities had implemented the reform, there was no longer any difference in the mean responses to the two manipulation check questions.

S5: Alternative estimation methods

In the analyses conducted below, I show that the key findings presented in table 6.2 are robust to employing two alternative estimation methods. These methods relax some of the assumptions made in order to estimate the models in table 6.2, and accordingly, they provide a more complete picture of the statistical evidence for the key conjecture of the analysis: that voters in the treatment municipalities held their mayoral party more electorally accountable for unemployment services than voters in the control municipalities.

The models estimated above did not take the hierarchical structure of the data – individual voters nested within municipalities – fully into account. In order to do this, I estimate a set of mixed effects multilevel logit models with the same configuration of variables used in table 6.2. Estimates from these models are presented in table S.6.6. The important estimates remain practically unchanged, although the standard error of the estimates increase slightly. Most importantly, the difference in AMEs remains statistically significant in three out of four models ($p \approx 0.05$). The logit interaction coefficients also remain statistically significant, although only at the ten percent level.

The tests used to assess the statistical significance of the interaction terms and differences in AMEs in table 6.2 rely on a number of parametric assumptions. To get around these assumptions, I tried to derive the statistical significance using a form of randomization inference; a non-parametric method (cf. Gerber and Green, 2012). In particular, I used the following procedure:

1. Draw a random sample of 14 municipalities, and create a dummy which was equal to one if the respondent lived in one of these randomly drawn municipalities.
2. Estimate the models reported in column 1-4 of table 6.2, but substituting the actual treatment variable for the dummy variable created in (1).
3. Store the estimated interaction effect between the simulated treatment dummy and unemployment performance obtained for each logit model estimated in (2).
4. Derive the the average marginal effect (AME) of unemployment performance in the simulated treatment and control municipalities for each of the models estimated in (2) and store the difference in AMEs.

Table S.6.6 Multi-level logistic regression of probability of voting for the mayoral party

	(1)	(2)	(3)	(4)
Unemployment performance	0.84*	0.84*	0.92*	0.88*
	(0.24)	(0.25)	(0.37)	(0.36)
Treatment	-0.60	-0.58	-0.49	-0.64
	(0.41)	(0.44)	(0.45)	(0.43)
Treatment * Unemployment performance	1.02 ⁺	1.02 ⁺	1.28 ⁺	1.36*
	(0.55)	(0.59)	(0.66)	(0.61)
Administration controls municipality	-0.31	-0.42 ⁺	-0.44 ⁺	-0.41 ⁺
	(0.21)	(0.21)	(0.24)	(0.24)
Housing performance	0.54 ⁺	0.53 ⁺	-0.01	0.07
	(0.30)	(0.32)	(0.40)	(0.40)
Elderly performance	0.83*	0.92*	1.06*	1.05*
	(0.23)	(0.25)	(0.31)	(0.30)
Sociodemographic controls		✓	✓	✓
Political controls			✓	✓
Municipal-level variables				✓
AME (Control)	0.17	0.17	0.10	0.09
	(0.05)	(0.05)	(0.04)	(0.04)
AME (Treatment)	0.36	0.35	0.23	0.24
	(0.08)	(0.09)	(0.06)	(0.05)
Difference (T-C)	0.18	0.18	0.14	0.15
<i>p-value of difference</i>	0.05	0.07	0.05	0.02
Log likelihood	-911.88	-881.73	-553.65	-550.06
Observations	1522	1500	1500	1500

Robust standard errors clustered on municipality in parentheses.

⁺ $p < 0.10$, * $p < 0.05$

5. Repeat (1)-(4) 10,000 times resulting in 10,000 unique interaction coefficients and AME-differences for each model.
6. Calculate *p-values* for each model by looking at the proportion of simulated logit coefficients and AME estimates which are *larger* than the ones estimated for the actual treatment and control municipalities.

A random sample of the 10,000 simulations is plotted in figure S.6.2 along with the calculated *p-values*. These *p-values* signify how likely it is to get an interaction or difference in AMEs of the size estimated in table 6.2 or larger *if* there was no effect of being assigned to implement the labor market reform for any of the municipalities (a sharp null). The *p-values* do become slightly larger using this method, however, the *p-values* are still below 0.1 and thus reflect that the observed difference in the weight voters put on unemployment service between treatment and control municipalities is unlikely to have occurred by chance.

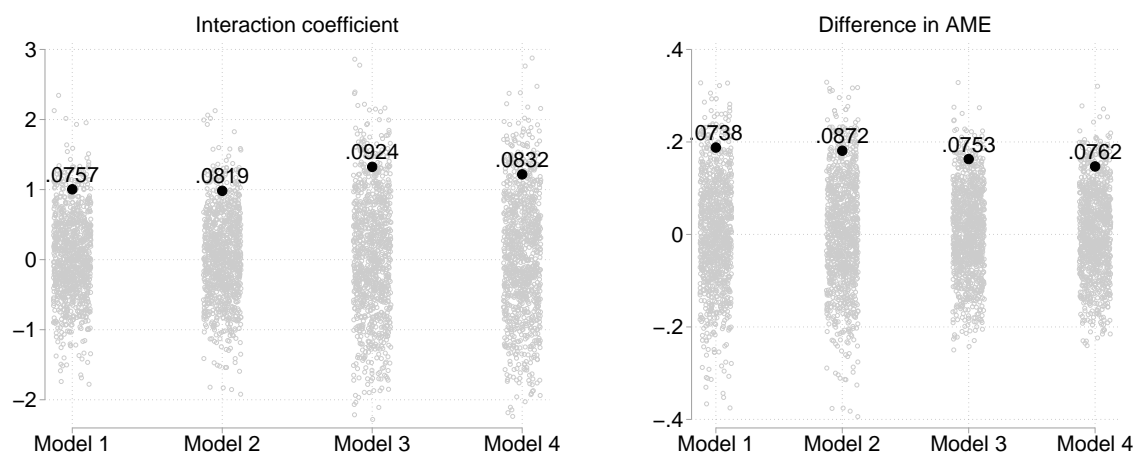


Figure S.6.2 A sample ($n=1,000$) of the simulated differences in AMEs and interaction effects from each of the four different logit models estimated in table 6.2. These are computed using randomization inference (RI). The black dot signifies realized outcome, taken from table 6.2, and the number attached to it is the RI *p-value*.

S6: Difference-in-difference

Conducting a difference-in-difference analysis is complicated by a few factors. Even though the key unemployment performance and vote intention questions were asked in both surveys, there is not a large overlap between the datasets when it comes to the control variables used in table 6.2. As such, I cannot estimate a model with as large a number of controls, however, this problem is somewhat offset by the difference-in-difference approach's ability to control for any pre-treatment differences between treatment and control municipalities. A more serious challenge to including the 2005 data relates to the fact that some municipalities were in the process of being amalgamated due to the large reform which was implemented in 2006 (cf. figure 6.1). As a result, almost half of the respondents voted in an amalgamated municipality, which was different from the one where their incumbent mayor had been elected, blurring patterns of accountability. I deal with this problem by by defining the dependent variable in '05 as voting for the party which had the mayoralty in the voter's existing (old) municipality. Even so, these amalgamations impede the strength of the analysis.

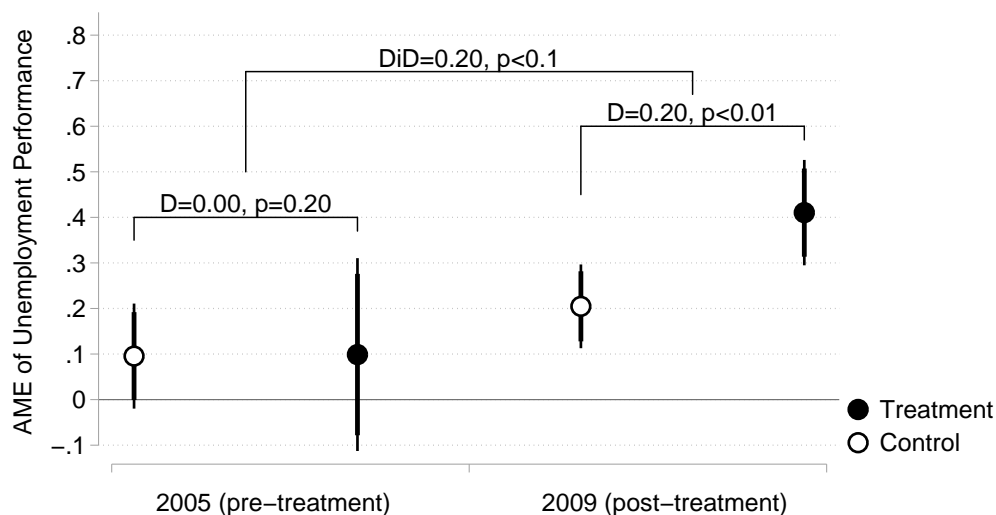


Figure S.6.3 Average Marginal Effects of unemployment performance on probability of voting for the mayoral party across treatment status and time period. Derived from logistic regression model described in the text; McFadden $R^2 = 0.032$, $n = 2,582$. Wald tests used to compare the different AMEs. The vertical lines are 90 pct. (thick) and 95 pct. (thin) confidence intervals.

In figure S.6.3, I show the AMEs of unemployment performance on support for the mayoral party in treatment and control municipalities in both 2009 and 2005. The AMEs are derived from a logistic model estimated on a pooled dataset. This model sets voting for the mayoral party as a function of a three-way interaction between unemployment performance, treatment status and time period ('09 versus '05). The model also controls for housing and elderly care performance as well as for whether the mayor is of the same party as the national government. To take the different patterns of accountability across amalgamated and continuing municipalities into account (cf. above), I allow all performance variables to have different slopes depending on whether voters lived in a municipality which was amalgamated.

As can be seen from figure S.6.3 there is no difference in the effect of voters evaluation of unemployment services on support for the mayoral party across treatment and control municipalities in 2005. Accordingly, before the reform of labor market regulation was implemented, there was no apparent difference in electoral accountability across treatment and control municipalities. In 2009, however, when the treatment municipalities had gotten more political responsibility for unemployment services, there is a statistically significant difference. The difference in difference estimate is only statistically significant at the ten percent level. The slight drop in statistical significance can be explained by the extra estimation error introduced by including the more noisy 2005 data.

S7: Analyzing additional policy areas

In this section, I examine differences in electoral accountability across the treatment and control municipalities for some additional policy areas. As such, I investigate whether voters in the treatment municipalities were more likely to electorally punish and reward the mayor for quality of services in nine different policy areas, which were not affected by the reform of labor market regulation (including the two examined in figure 6.3). In particular, I use the logit model presented in column 4 of table 6.2 as a template, swapping the unemployment performance variable for one of the alternative policy variables. I do this for all policy variables. For each of these nine new models, I then derive the AME of the policy variable on voters' propensity to vote for the mayoral party in both the treatment and in the control municipalities. Finally, I test the AME in the treatment municipalities against the AME in the control municipalities using a Wald test. The results of these analyses are reported in table S.6.7.

Table S.6.7 Differences across treatment and control for other policy outcomes

Policy Area	Treatment	Control	Standard error	p-value
Housing	0.05	0.04	0.09	0.85
Daycare	0.08	0.08	0.10	0.94
Recreation	0.01	0.08	0.08	0.44
Schools	0.08	0.03	0.08	0.56
Library	0.04	0.09	0.07	0.47
Culture	0.19	0.09	0.09	0.28
Business	0.18	0.10	0.10	0.43
Elderly Services	0.14	0.11	0.09	0.79
Health Services	0.17	0.08	0.09	0.29

The models from which the average marginal effects are derived include the full set of controls.

As is revealed by looking at the right-most column of table S.6.7, the AME of voters' assessment of the quality of the services provided in these nine different policy areas do not significantly differ across treatment and control municipalities.

S8: No Evidence of Increases in Partisan Motivated Reasoning

An alternative explanation for our findings is that there are voters in the municipalities who (dis)like the mayoral party, and when they find out that their mayor has become more responsible for unemployment services, they increase (or decrease) their estimate of service quality in this area accordingly. If this is the case, voters' satisfaction with unemployment services should be more strongly correlated with *past* support for the mayor in the municipalities where the mayor got more responsibility for unemployment services.

In order to examine this possibility, we re-estimate the four logistic regression models from table 6.2 using self-reported support for the (current) mayoral party at the previous election as the dependent variable. (The models are thus only estimated using respondents who said that they could remember which party they voted for at the last election.) Table S.6.8 presents the results from these analyses. In these models, which predict past voting, the interaction effect between treatment status and unemployment performance is negligible and statistically insignificant. So is the difference in AMEs across treatment and control municipalities. From this, we can conclude that the increased correlation between satisfaction with unemployment services and support for the mayor is not be driven by voters who already supported the mayor at the last election becoming more satisfied with unemployment services, or by voters who did not support the mayor becoming less satisfied.

More broadly, these analyses show that there is no sign of increases in partisan motivated reasoning when it comes to how satisfied voters are with unemployment services in the treatment municipalities. This corroborates the initial conclusion that voters hold their mayor more electorally accountable for the quality of unemployment services.

Table S.6.8 Logistic regression of voting for the mayoral party at the *last* election

	(1)	(2)	(3)	(4)
Unemployment performance	0.53*	0.55*	0.45	0.43
	(0.21)	(0.23)	(0.32)	(0.32)
Treatment	-0.18	-0.20	-0.17	-0.14
	(0.28)	(0.28)	(0.34)	(0.34)
Treatment * Unemployment performance	0.02	0.05	0.00	-0.03
	(0.51)	(0.52)	(0.76)	(0.76)
Administration controls municipality	-0.18	-0.28	-0.09	-0.11
	(0.19)	(0.20)	(0.26)	(0.26)
Elderly performance	0.10	0.22	-0.03	-0.04
	(0.20)	(0.21)	(0.25)	(0.25)
Housing performance	0.86*	0.74*	0.84*	0.87*
	(0.28)	(0.28)	(0.35)	(0.36)
Sociodemographic controls		✓	✓	✓
Political controls			✓	✓
Municipal level variables				✓
AME (Control)	0.12	0.12	0.06	0.06
	(0.05)	(0.05)	(0.04)	(0.04)
AME (Treatment)	0.12	0.13	0.06	0.05
	(0.11)	(0.10)	(0.10)	(0.10)
Difference (T-C)	0.00	0.01	-0.00	-0.01
<i>p-value of difference</i>	1.00	0.96	0.98	0.95
Pseudo R ²	0.02	0.04	0.34	0.34
Log likelihood	-965	-937	-644	-642
Observations	1476	1461	1461	1461

Robust standard errors clustered on municipality in parentheses.

+ $p < 0.10$, * $p < 0.05$

S9: No Evidence of Short-lived priming

An alternative explanation for why voters increased the weight they put on unemployment services in the election following the reform of labor market regulation is that the reform briefly primed this issue (Krosnick and Kinder, 1990). That is, the reform could have led to a brief period of increased political attention to unemployment services in the treatment municipalities, which, in turn, could have lead voters to rely more on their evaluation of the quality of these services, when deciding whom to vote for. I offer two pieces of evidence which counter this alternative explanation.

First, from the analysis conducted in section S4 (cf. table S.6.5), we know that voters in the treatment municipalities, where the mayor got more responsibility for unemployment services, did in fact believe the mayor was more responsible for what happened in the municipality. This should increase our confidence that the difference in behavior between the treatment and control electorates was, at least in part, due to differences in beliefs about political responsibility, and therefore not solely due to short-lived priming.

The second piece of evidence against the short-lived priming explanation can be found by examining voters' political agenda in the 2013 municipal election. In 2013 all municipalities had implemented the reform, leaving responsibility balanced across treatment and control municipalities, but the control municipalities differed from the treatment municipalities in that they had implemented the reform recently (in 2010). Accordingly, if the short-lived priming explanation is correct, unemployment services should be higher on the voters' agenda in the 84 control municipalities in 2013.

To examine this empirical implication, I measured voters' agenda in the different municipalities at the 2013 election. Specifically, I examined open-ended answers given to two questions: "When thinking back on the campaign in your own municipality, what was the most debated issue?" and "What did you think should have been the most debated issue?" Arguably, the content of these open-ended responses should give us an indication of whether unemployment services were higher on the agenda in some municipalities than in others. To analyze the responses, I coded whether they mentioned at least one of four stemmed key words related to unemployment services.¹⁶ I use this indicator as a rough measure of the extent to which unemployment services were on the voters' agenda in the different municipalities. If

¹⁶The four key-words were: *arbedj*, *job*, *erhverv*, *beskæf*. The key words are stemmed versions of different synonyms for work (i.e. work, job, employ, occupation). For each of the open ended answers, about ten percent of respondents used at least one key word.

the short-lived priming explanation is accurate, unemployment services should be higher on the voters' agenda in the control municipalities. This does not seem to be the case. I find very small and statistically insignificant differences in the percentage which used at least one key word for both the question about what voters believed was on the agenda ($d = 0.07$ percentage points, $p = 0.34$), and the question about what voters thought should have been on the agenda ($d = 0.04$ percentage points, $p = 0.37$). In sum, there is no evidence for the short-lived priming explanation.

Chapter 7

Clarity of Political Responsibility and Incumbent Behavior

with Asmus Leth Olsen

A large body of literature suggests that clarity of political responsibility moderates the extent to which voters hold incumbent politicians electorally accountable for policy outcomes (i.e., retrospective voting). To the extent that incumbents' actions are shaped by voters' ability to select incumbents who act in voters' best interest, clarity of responsibility should also affect incumbent behavior. In particular, we argue that because diffusion of political responsibility dilutes effective electoral accountability, clarity will increase the extent to which incumbent politicians pursue the type of policy outcomes that their electorate prefers. We examine the empirical viability of this argument by studying tax policy in Danish municipalities. Exploiting an electoral discontinuity in clarity of responsibility, we show that high levels of clarity goes hand in hand with local incumbents doing what the average voter prefers: lowering taxes.

Elections provide voters with an opportunity to unseat politicians who will not or cannot adhere to the will of the people (Alesina and Rosenthal, 1995; Ashworth, 2012; Besley, 2006; Besley and Case, 1995a; Ferejohn, 1986). Some elections, however, seem to provide voters with a much better opportunity to hold politicians effectively accountable. In some elections, it is pretty clear that the incumbent is responsible for the divergence between what voters prefer and the political reality, while in other elections it might be quite difficult for voters to find out who is responsible for any such divergence. If, for instance, the incumbent is a prime minister in a coalition government, it is difficult for voters to know whether the policies enacted and outcomes realized under this incumbent is the responsibility of the prime minister or whether it is the responsibility of the coalition partners (Hobolt, Tilley and Banducci, 2013; Whitten and Palmer, 1999).

More generally, diffusion of political responsibility challenges the effectiveness of electoral control: if voters do not know whether their incumbent is responsible for the state of important policy outcomes, then their ability to elect incumbents who are able and willing to pursue the type of policy outcomes they prefer is compromised (Achen and Bartels, 2016; Anderson, 2007). This is the premise behind a number of studies of how voters react to differences in clarity of political responsibility. Broadly speaking, these studies have found that in countries where economic and political institutions disperse political responsibility, voters are less likely to hold incumbents electorally accountable for how their country is doing economically (Anderson, 2000; Duch and Stevenson, 2008; Powell and Whitten, 1993; Van der Brug, Van der Eijk and Franklin, 2007).

An interesting implication of this pattern in voter behavior is that in elections where clarity of responsibility is low, electoral incentives for incumbents to provide the type of policy outcomes voters prefer are going to be correspondingly low. This might have a substantial effect on incumbent behavior. Yet, in spite of this, the link between clarity of responsibility and incumbent behavior remains largely unexamined in the existing literature (for a notable exception, see Tavits, 2007). Instead, studies of clarity of responsibility have almost exclusively examined voter behavior (Healy and Malhotra, 2013, 301), whereas studies of how electoral incentives shape incumbent behavior have focused on other factors, such as the competitiveness of elections (Besley, Persson and Sturm, 2010), the quality and extent of information available about the incumbent (e.g., Alt and Lowry, 2010; Snyder Jr and Strömberg, 2010) and whether the incumbent is eligible for re-election (e.g., Alt, De Mesquita and Rose, 2011; Besley and Case, 1995a).

In this article, we explore whether clarity of responsibility does affect incumbent behavior. In particular, since high levels of clarity of political responsibility strengthens effective electoral accountability, we argue that high levels of clarity of responsibility will make it more likely that incumbents enact policies and pursue policy outcomes that are aligned with what the average voter prefers (i.e., policy outcomes will be more aligned with what more voters prefer).

We examine the empirical viability of this argument by studying income tax policy in Danish municipalities. We leverage three important aspects of this context. First, we use local income tax rates, a salient policy outcome, as a metric of whether incumbent politicians do what the average voter prefers. In particular, we show that raising income taxes is unpopular with the average voter, finding that if a municipality raises taxes, the largest party loses electoral support at the subsequent election (see also Bhatti, Hansen and Olsen, 2012; Frandsen, 1998). Second, we use single party majority status in municipal city councils as a measure of clarity of responsibility. Historically, the largest party in the city council has controlled a majority of the seats in one out of four Danish municipalities. If the largest party controls a majority of the seats, then this party can set the municipal income tax rate without coalescing with other parties. If the largest party only controls a plurality of the seats, then it needs to cooperate with at least one other party in order to set the income tax rate. This makes single party majority status a good indicator of clarity of responsibility for tax rates. Third, we exploit an electoral discontinuity in order to identify the causal effect of single party majority status on income tax policy. In particular, we use a close elections regression discontinuity design, comparing municipalities where the largest party narrowly won a majority of the seats in the city council to those where the largest party narrowly lost a majority.

Guided by the literature on how voters hold incumbents electorally accountable, we explore two potential mechanisms through which single party majority status might influence the tax rate set by the city council (Alt, De Mesquita and Rose, 2011; Duch and Stevenson, 2008). First, a sanctioning – or moral hazard – mechanism which suggests that parties with a majority will refrain from raising taxes to avoid getting punished by the voters. Second, a selection mechanism that suggests voters will have an easier time (s)electing local politicians who are willing and able to lower taxes, if it is clear who is responsible for tax policy. Studying the effects of gaining a single majority on tax policy, we find robust evidence of the selection mechanism, but no evidence of the sanctioning mechanism. As such, we find that single party majority status *does* lead to lower taxes, but this is not because the largest party

adjusts their behavior in anticipation of being more harshly punished for raising taxes, but because the presence of a single party majority makes it possible for voters to shift power away from the type of parties who do not keep income taxes low and towards the type of parties who do.

This study broadens the scope of the literature on clarity of political responsibility – a body of research which has generally remained observational and paid little attention to incumbent behavior (Powell and Whitten, 1993; Silva and Whitten, 2017; Tavits, 2007). Further, with a strong consensual and non-partisan political culture, where decisions often are based on over-sized multi-party coalitions (Blom-Hansen, 2002; Serritzlew, Skjæveland and Blom-Hansen, 2008), Danish municipalities constitutes a hard case in contrast to a literature that typically focuses on two party systems, where the political competition is stronger, and formal clarity of responsibility is more clearly reflected in the norms and traditions of the political system (e.g., Alt, De Mesquita and Rose, 2011; Besley, Persson and Sturm, 2010). By demonstrating that dispersion of political responsibility leads to the adoption of policies that are unpopular with the average voter, this study also re-emphasizes that elections will not always serve as an effective check on incumbent politicians (see also Ashworth, Bueno de Mesquita and Friedenberg, 2017; Healy and Lenz, 2014; Healy and Malhotra, 2009; Huber, Hill and Lenz, 2012). In particular, our results suggest that formal electoral accountability only translates into effective electoral control if the political context provides a certain degree of clarity of responsibility.

The Argument and The State of the Art

Voters often look back at how incumbent politicians have performed in office when deciding whether to re-elect them (Fiorina, 1981; Key, 1966; Kramer, 1971; Lewis-Beck, 1990; Van der Brug, Van der Eijk and Franklin, 2007). While this type of retrospective voting is prevalent to some extent in most elections (Lewis-Beck, 1990), its exact scope varies substantially from election to election (Duch and Stevenson, 2008). One popular explanation for why retrospective voting varies is that electoral contexts vary in the extent to which they assign clear responsibility for political performance to the incumbent government. In particular, this clarity of responsibility explanation suggests that “voters’ assignment of responsibility to the government [...] will strongly reflect the nature of policymaking in the society and the coherence and control the government can exert over that policy” (Powell and Whitten, 1993, 398). This explanation has been empirically powerful with studies showing that factors like

coalition government (Hobolt, Tilley and Banducci, 2013; Nadeau, Niemi and Yoshinaka, 2002) and federalism (Anderson, 2006; Arceneaux, 2006; León, 2011) moderates retrospective voting by dispersing responsibility for policy-making across multiple actors. Similarly, other studies have found that, more broadly, an incumbent's ability to influence the welfare of the electorate moderates the extent of retrospective voting (e.g., Duch and Stevenson, 2008; Hellwig and Samuels, 2007; Hellwig, 2001).

At the same time, research on the nature and scope of electoral accountability has found that the presence of retrospective voting behavior constrains incumbent behavior in two ways (Ashworth, 2012). First, if incumbents know that they will not be re-elected by retrospective voters unless they produce the type of policy outcomes these voters prefer, incumbents will exert more effort to produce such outcomes. This is sometimes called a sanctioning effect (e.g., Ferejohn, 1986). Second, by only retaining incumbents who produce policy outcomes they like, retrospective voters shift the composition of incumbents towards the types who produce outcomes that they like, and away from the types who produce outcomes that they do not like. This is sometimes called a selection effect (e.g., Fearon, 1999).¹ While the underlying mechanisms are distinct, both selection and sanctioning have a similar effect on incumbent behavior, in that they make incumbents pursue policy outcomes that voters want.

This brings us to our key argument: Since clarity of responsibility moderates retrospective voting by making it either easier or harder for voters to find out who is responsible for policy outcomes, we expect that clarity of responsibility will also moderate incumbent behavior by making incumbents' re-election chances more or less dependent on the quality of the policy outcomes they oversee. In particular, we expect that if clarity of responsibility for a certain policy outcome increases, then incumbents overseeing this outcome will try harder to align the state of the outcome with what the average voter prefers.²

How might this work in practice? Imagine a minority government who cannot pass legislation without bringing in opposition parties, blurring the extent to which the government is actually responsible for the legislative output. If some piece of

¹The terminology used here is from Duch and Stevenson (2008), but the division into these two types of effects is widely used (e.g. Alt, De Mesquita and Rose, 2011; Ashworth, 2012; Ashworth, Bueno de Mesquita and Friedenberg, 2017)

²When we say incumbents will try to align the state of the policy outcome with what the average voter prefers, we mean that they will pursue outcomes that leaves a majority of the voters better off. Accordingly, we expect that a majority of voters would prefer the policy outcomes pursued by incumbents when clarity of responsibility is high to the policy outcomes pursued by incumbents when clarity of responsibility is low.

unpopular legislation gets passed into law, voters will have a hard time figuring out whether the governments or the opposition is responsible. Perhaps, the voters reason, the legislation would have been much better if the government did not have to deal with the opposition. The government knows that voters will not be able to determine the extent to which they are responsible, and this reduces their incentive to design the best possible legislation. If voters will not credit us, the government thinks, why put in the effort? At the same time, governments who are actually interested in producing good legislation will tend to get re-elected at near the same rate as governments who are not particularly interested in producing good legislation, because the oppositions' interference makes it difficult for voters to find out whether the government is interested in producing good legislation. Contrast this with a situation where the incumbent government can pass legislation without coalescing with the opposition. If unpopular legislation is passed into law, voters will know who to blame. The government knows this and therefore has a strong incentive to produce popular legislation, and governments who pass unpopular legislation will be re-elected at a much lower rate than those who pass popular legislation. In sum, the popularity of the legislation pursued by the government will depend on whether the government is clearly responsible for the legislation or not.

Interestingly, only a few studies have looked at the link between clarity of responsibility and incumbent behavior (e.g., Tavits, 2007), and no study has looked at whether clarity of responsibility affect what policies incumbents pursue. Even so, a number of different findings from the previous literature motivates and informs our argument.

One set of studies, which is relevant in this regard, compare incumbents who are term-limited with incumbents who are not term-limited in order to study the effect of re-election incentives on incumbent behavior. Using this method different studies have found that being up for re-election makes it less likely that incumbents engage in corruption (Ferraz and Finan, 2011), more likely that incumbents enact popular economic policies (Besley and Case, 1995b) and leads to the provision of better social and economic conditions (Alt, De Mesquita and Rose, 2011; De Janvry, Finan and Sadoulet, 2012). These studies tell us that re-election incentives can have a size-able effect on incumbent behavior, making it plausible that clarity of responsibility, which arguably moderates these re-election incentives, might have an effect as well.

Another set of studies looks at the role of information. Examining variations in the level of media scrutiny (e.g., Howell, 2005; Snyder Jr and Strömberg, 2010), or formal rules that make policy outcomes more transparent (Alt, Lassen and Skilling,

2002; Alt and Lowry, 2010), these studies have found that incumbents are more likely to exert an effort to align policy outcomes with what voters prefer when voters have more information about the state of these policy outcomes. Even though this article is concerned with variations in political responsibility and not information, these studies lend credibility to the idea that incumbents take stock of their electoral context when deciding what policy outcomes to pursue.

Finally, a number of studies have found that unified government at the state level in the US is correlated with incumbent behavior. This is relevant in the context of our argument, because unified government increases clarity of responsibility, and has been shown to moderate retrospective voting (Lowry, Alt and Ferree, 1998, although see Nadeau and Lewis-Beck, 2001; Norpoth, 2001). The results from these studies, however, are mixed. Some find that unified government leads governors to enact policy that is more in accordance with their party's ideology (Alt and Lowry, 1994; Clarke, 1998), whereas others find that unified government make governors more responsive to changes in their state's policy mood (Coleman, 1999). Still others find that unified government makes little to no difference for responsiveness (Lax and Phillips, 2012). The inconsistency of these results challenge our argument – since unified government increases clarity of responsibility, we would expect that unified government makes incumbents uniformly more responsive. However, these studies tend to suffer from problems related to causal inference: states that have unified government are very different from states that have divided government on a number of different parameters (e.g., ideological cohesion of the electorate). This might, in part, explain the inconsistent results. Below, we make sure to examine a context where it is possible to derive a more plausibly causal estimate of how clarity of responsibility affects incumbent behavior.

Taken together, this brief review of the existing literature shows that while there is plenty of research which would suggest clarity of responsibility could make incumbents more responsive to the electorate, there is little research directly on this topic, and the studies that comes closest to studying this question have mixed results.

Empirical Context: Tax Policy in Danish Municipalities

To examine the empirical tractability of our theoretical argument, we zoom in on Danish municipalities. Denmark is a decentralized welfare state where municipalities can affect their local revenue and set a yearly budget. Municipal tasks and services include the core welfare services of the Danish welfare state and municipal

spending amounts to 35 percent of GDP.³ In 1970 a municipal reform was introduced which dramatically reduced the number of municipalities. In 2007 the local level experienced yet another major reform which reduced the number of municipalities once again. The time in between these two reforms is the period we study.⁴

This context provides us with three things that we need in order to explore whether clarity of responsibility makes incumbents pursue policy outcomes preferred by the average voter. First, a policy outcome that is controlled by the municipal city council and which is unpopular with the average voter: municipal income taxes. As such, the municipal income tax rate is set by a simple majority in the city council, and municipal voters show an electoral preference for lower municipal income tax rates. Second, meaningful variation in clarity of responsibility for tax policy across elections: single party majority status. Sometimes the largest party in the city council controls a majority of the seats in the council, and sometimes the largest party only controls a plurality of the seats. This means that sometimes the largest party can set taxes on their own, leaving them clearly responsible for tax policy, and sometimes they need to negotiate with other parties, leaving political responsibility for tax policy less clear. Third, and perhaps most important, a way of identifying the causal effect of gaining a single party majority on tax policy: a close elections regression discontinuity design. As such, by comparing the tax rates in municipalities where the largest party fell just short of getting a majority of the seats in the city council with municipalities where the largest party narrowly won a majority, it should be possible to unearth an unbiased estimate of the causal effect.

Below we discuss the nature of income taxes in Danish municipalities and the difference between single party majorities and pluralities, detailing how these aspects of local government will help us explore our theoretical argument. Afterwards, we derive some specific hypotheses about how single party majority status affects tax policy.

³The tasks include primary education, child care and care for elderly people, libraries, local sports facilities and other cultural activities, granting and payment of cash assistance, anticipatory pension and certain other social benefits, job activation and employment projects for non-insured unemployed persons, public utilities, environmental measures as well as emergency services.

⁴We exclude the municipalities of Copenhagen and Frederiksberg which in this period had a different income tax structure that combined the regional and local income tax. The number of municipalities vary a bit across time due to amalgamations.

The Political Features of Local Income Taxes

Local governments in Denmark enjoy a large degree of income tax discretion. The local income tax rate is determined by the city council in connection with the yearly budget negotiations, which come to a vote in the fall of each year (Blom-Hansen, 2002).⁵ The local income tax rate is salient – all citizens are informed about the tax rate via their early tax returns, and the media will also frequently report on tax rate changes; it is broad based – the tax rate is the same for all levels of income and cannot be bracketed; and it has a large impact on voters' financial situation – tax rates typically vary from ten to 20 percent.

Increasing the local income tax rate is unpopular with the electorate. As such, unlike in the international literature, where there is mixed evidence regarding the relationship between local income taxation and support for local incumbents (Besley, 2006; Boyne et al., 2009; Lowry, Alt and Ferree, 1998; Niemi, Stanley and Vogel, 1995; Peltzman, 1992), the Danish literature on this topic has consistently found that increasing income taxes carry an electoral penalty (Bhatti, Hansen and Olsen, 2012; Frandsen, 1998). In section S14 the supplementary materials, we replicate these analyses. Using lagged dependent variable and difference-in-difference models, we find, just like previous analyses have, that the largest party in the city council is punished by the voters for raising taxes, showing that the average voter has an electoral preference for lower taxes. We also find that this effect is pretty stable across the period under study, with no statistically significant deviations from election to election.

In sum, local income taxes are set by the city council, they are salient to voters, and raising income tax rate carries an electoral penalty. These features make income tax policy in the Danish municipalities interesting in the context of our theoretical argument, because we can index the extent to which incumbents are responsive to their electorates preferences for low(er) taxes, by examining whether they do, in fact, keep income taxes low.

⁵In theory municipalities are allowed to set the tax rate at any level. Formally, tax discretion is limited by national agreements between the central government and the association of local governments. They reach a common non-binding agreement about how much the average tax rate across all municipalities is allowed to increase. However, in practice, these agreements have had little impact on the tax level in each individual municipality (Pedersen and Jensen, 2004).

Single Party Majorities

The city council is the primary form of local government in Denmark. Members are elected at proportional single district elections, which are fixed to take place every four years and do not usually coincide with elections at the national or EU level. Turnout is high with an average of around 70 percent. The size of the city councils in the primary municipalities goes from 9 to 31 seats.

After each election the mayor is appointed by a majority in the city council. This means that after some elections the mayor will be appointed by just one party, a single party majority, whereas sometimes several parties will have to agree on a mayor. In these cases, the mayor will often end up being from the party, which has a plurality of the seats. Just as important for present purposes, however, is that when a party gets a single party majority they can set the income tax rate without coalescing with other parties, increasing clarity of responsibility for tax policy. About one in four elections result in a single party majority.

When a party wins a majority of the seats it will generally be the dominant political force in the municipality (Olsen, 2013). Further, while there has been very little research on the effect of single party majorities in a Danish context, Serritzlew (2005) concludes that “if the mayor’s party has an absolute majority, the need for compromise is limited, and implementation will not be challenged politically” (p. 422). This is consistent with our argument that gaining a single party majority enhances clarity of political responsibility.

Taken together, we think single party majority status is a valid indicator of clarity of responsibility. This is further corroborated by the fact that incumbents’ ability to set fiscal policy without coalition partners or support from other parties is one of the most commonly used indicators of clarity of responsibility (e.g., Hobolt, Tilley and Banducci, 2013; Nadeau, Niemi and Yoshinaka, 2002; Powell and Whitten, 1993).

Hypotheses

What are our expectations with regards to the relationship between single party majority status and the tax policy pursued by the members of the city council? Above we argued that if it is clear who is responsible for a certain policy outcome, then it is more likely that incumbent politicians will try to deliver what the average voter prefers with respect to this policy outcome. Since the largest party’s responsibility for income taxes increases substantially if the largest party obtains a majority of seats in the city council, and since the average voter has a well documented electoral

preference for lower income taxes, we expect that single party majority status will lead to lower income taxes. In particular, one can imagine two empirically distinct ways that single party majority status affects tax policy.

One is that single party majority status leads the city council to pursue lower taxes, because if the largest party obtains a majority of the seats, they expect to be held more electorally accountable for tax policy, and therefore they have a stronger electoral incentive to lower taxes. Following the literature on electoral accountability (e.g., Ashworth, 2012), we can call this a sanctioning hypothesis (Besley, 2006; Ferejohn, 1986). It is important to note, that this sanctioning hypothesis is premised on the idea that it takes effort for the largest party to lower or refrain from raising taxes. If it took no effort, there would be no sense in not lowering taxes, even if the reelection incentives were weak. However, it seems reasonable to assume that lowering taxes require the largest party to exert some effort. In particular, if it were to cut taxes, the largest party would need to find an alternative source of revenue or it would need to cut spending.

Sanctioning hypothesis: If a party obtains a majority of seats in the city council, then the city council will adopt lower income taxes *before* the next election.

When the largest party obtains a single party majority, they can set tax policy without interference from other parties, making the tax rate a better indicator of this party's willingness and ability to keep taxes low. To the extent that voters use this improved indicator of the incumbents 'type' when allocating electoral support in the next election, they will shift power away from parties that raise taxes and towards parties that lower taxes, which should, all else equal, lead to a city council that adopts lower taxes. This is the second way single party majority status might affect tax policy. Following the literature on electoral accountability, we can call this a selection hypothesis (Besley, 2006; Duch and Stevenson, 2008; Fearon, 1999).⁶ It is important to note, that the selection hypothesis does not predict that the change in tax policy will kick in immediately, rather, it predicts that the effect will work through changes to the composition of the city council, meaning that the effect will not materialize until after the next election.

⁶This selection hypothesis is premised on the idea that some parties are low-tax types, who will always work to lower taxes, whereas others are high-tax types, who will always work to raise taxes. If parties did not differ in this respect, voters could not affect the tax rate by affecting the composition of the city council.

Selection hypothesis: If a party obtains a majority of seats in the city council, then the city council will adopt lower income taxes *after* the next election.

The two selection and sanctioning hypotheses are not mutually exclusive. One can easily imagine both selection and sanctioning mechanisms operating simultaneously (for models that integrate sanctioning and selection effects, see Alt, De Mesquita and Rose, 2011; Besley, 2006). If sanctioning and selection mechanisms are at work at the same time, then incumbents' motivation might change slightly, but the basic predictions should remain the same.⁷

Figure 7.1 lays out how the two hypotheses relate single party majority status to tax policy. Following the election of a single party majority at $t = 0$ the sanctioning hypothesis predicts that the incumbent party will lower taxes before the next election at $t = 1$, because the single party majority incumbent knows that the voters will hold them more electorally accountable for the municipal tax rate at $t = 1$. Following the election featuring a single party majority incumbent at $t = 1$, the selection hypothesis predicts that the new city council will lower taxes before the next election at $t = 2$, because voters were able to find out whether the single party majority up for election at $t = 1$ was a low-tax or a high-tax type, and shifted power away from the party if it was the latter.

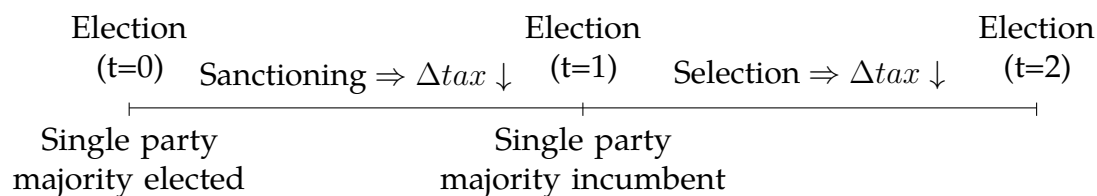


Figure 7.1 A timeline of what the different hypotheses predict with respect to the effect of single party majority status on tax policy.

⁷In particular, when selection and sanctioning are at work simultaneously, incumbents who are high-tax types will try to mimic low-tax types in order to get re-elected when clarity of responsibility is high, leading to lower taxes before the next election. At the same time, however, the mimicking will make it harder for voters to sort low-tax types from high-tax types, than in the case of pure selection. Even so, it seems likely that sometimes the effort needed for high-tax types to mimic low-tax types will be too high, and in these cases the high-tax types will end up setting the tax rate higher than the low-tax types. In these cases it will thus be possible for voters to distinguish between the two types, leading to lower taxes following the next election (these implications follow from the model laid out in Besley and Case, 1995a).

Identification Strategy: A Regression Discontinuity

In order to test our hypotheses we need to be able to identify the effect of single party majority status on the tax policy pursued by the city council. However, like all political institutions, assignment of single party majority status is endogenous to the political and socioeconomic makeup of the municipality (Aghion, Alesina and Trebbi, 2004; Besley and Case, 2000). For instance, electing a single party majority will be easier in smaller, rural municipalities where the electorate is more homogeneous. In these rural municipalities, however, voters will also tend to be more conservative, and therefore have a stronger preference for lower taxes. This poses a challenge for causal inference, as we might conflate the effect of single party majority status with the effect of having a more conservative electorate. More generally, we can say that the underlying factors which make single party majority status more likely might also make adoption of specific tax rates more likely.

To sidestep these inferential issues and estimate an unbiased causal effect, we use a close elections regression discontinuity (RD) design. That is, we compare tax rates in municipalities where the largest party narrowly won a majority of the seats in the city council with tax rates in municipalities where the largest party narrowly lost a majority of the seats. This means focusing on municipal elections where only a few votes were pivotal for giving the largest party a majority of the seats. When focusing on these close elections, we can reasonably assume that it is as-if random whether the largest party won a majority of the seats (for evidence of this, see Eggers et al., 2015). This as-if random assignment of single party majority status makes it possible to causally attribute differences in tax rates to the presence or absence of a single party majority (Lee, 2008; Skovron and Titiunik, 2015).

Identifying Closeness of the Elections

We identify close elections in a two-step process. In the first step, we single out municipal elections held between 1970 and 2001 where the largest party in the city-council ended up having a one-seat majority or was one seat short of obtaining a majority. This leaves us with 839 close elections out of a pool of 2,475 elections.⁸ Figure 7.2 illustrates this selection process. In the second step, we index the 839

⁸Municipalities with close elections tend to be smaller and more rural than the average municipality, but they resemble the average municipality with respect to the ideological make-up of the electorate and with respect to the tax rate. See section S1 of the supplementary materials for a table that presents these differences.

close elections according to *how* close they actually were. That is, we create a forcing variable that can tell us how many additional votes the largest party would have needed to win (lose) in order to have won (lost) the pivotal seat in the city council.

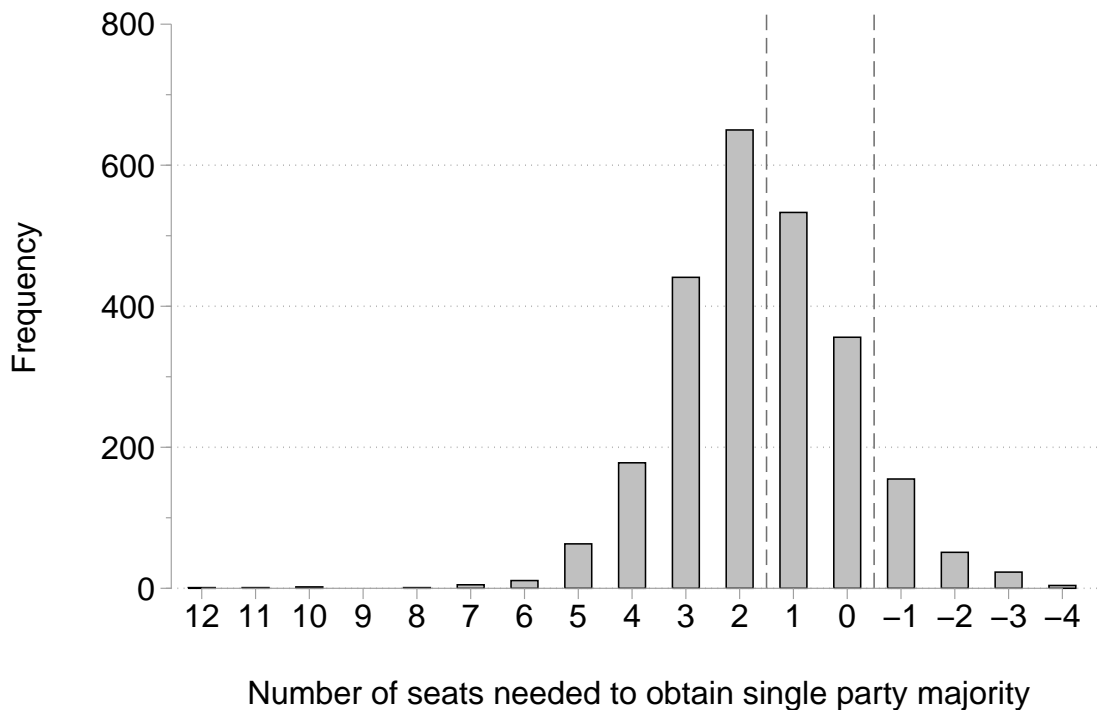


Figure 7.2 How many additional seats did the largest party need to obtain a majority? Only elections within the two dashed lines are used in our RD analyses.

Creating the forcing variable, however, is complicated by the fact that there is no joint electoral cut-off at which the largest party is always assigned a majority of the seats in the city council (e.g., 50 pct.). This is not to say that there is no exact cut-off at which majority is assigned in each election; the cut-off simply moves around from election to election. Sometimes it might be 42 pct. and sometimes it might be 45 or 48 pct.

The varying cut-off is a product of two features of the electoral system in the Danish municipalities. The first feature is that assignment of seats is based on a proportional divisor method. As a consequence, the number seats assigned to the largest party depends on the exact configuration of votes cast for the other parties up for election (Fiva, Folke and Sørensen, 2016; Folke, 2014; Freier and Odendahl, 2015). The second feature is that parties can form electoral coalitions (Cox, 1997). If parties decide to form an electoral coalition, which they often do, then seats are first

assigned to this coalition, and then to the individual parties. As a result, the number of seats assigned to each party depends on the configuration of electoral coalitions, the votes cast for the different electoral coalitions, and the votes cast for the different parties within each coalition.

To develop a forcing variable which takes these particularities of the electoral system into account, we first specified the exact distribution of votes across parties and electoral coalitions for each of the 839 close elections using election reports from Statistics Denmark. Next, we wrote a program that ran simulated elections to determine the number of votes (+/- 10) the largest party would have needed to either win/lose one seat (and thereby secure/lose their majority in the city council).⁹ Based on this, we measured the number of votes and the proportion of votes that the largest party in the city council would have needed to either win a majority or lose their majority. Figure 7.3 shows how these variables are distributed, that is, exactly how close the 839 close elections were. As can be seen from this figure, there are a large number of elections close to the cut-off (0) in our dataset. In the analyses below, we use the proportional measure as our forcing variable.

Indicators

Our key dependent variable is the municipal income tax rate. Because there are substantial temporal and cross-municipality differences in the tax rate (see section S1 of the supplementary materials for details), we use the benchmarking procedure suggested by Lee and Lemieux (2010, 331). This procedure makes it easier to compare tax rates across municipalities and across different time periods (i.e., it increases efficiency). Specifically, we adjust the income tax rate implemented in a given year by a given municipality by subtracting (1) the average municipality-specific tax rate (i.e., average rate across all years for that municipality) and (2) the difference between the average year-specific tax rate and the average tax rate across all years.¹⁰

⁹In particular, the program added or subtracted 10 votes from the actual vote total of the largest party, while holding the electoral support for the other parties constant, and then calculated the number of seats the largest party would have gotten given the electoral coalitions and the votes cast for other parties. It performed this calculation until the number of seats assigned to the largest party changed, and then reported how many times it had to add or subtract 10 votes before this change had happened. To write this program we used the `electools` package in Stata (Jaime-Castillo, 2008).

¹⁰Note that the average municipality-specific tax rate and the average year-specific tax rate are going to be balanced – in expectation – for the subset of close elections which we use to identify the effect of single party majority status on income taxes. Accordingly, this transformation is not used to get an unbiased estimate, instead, the transformation provides us with a more efficient comparison across time and space. Here is an example of how the adjusted tax rate is calculated. Say a municipality implements a 20 pct. income tax in 1980, that the average level of taxes this municipality has levied

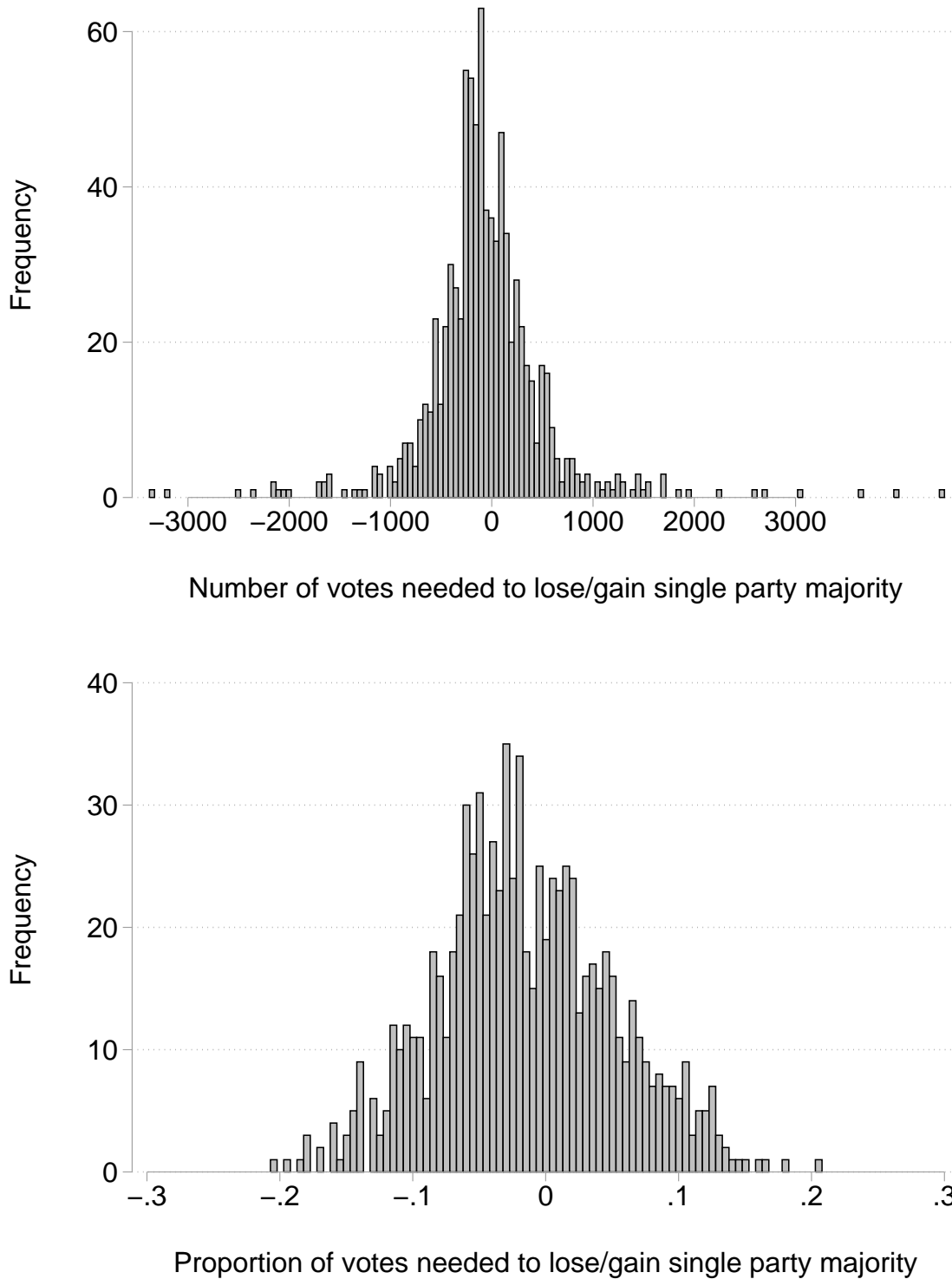


Figure 7.3 Density of forcing variable measured as number of votes and proportion of votes. Only calculated for the 839 elections where single party majority status would be reassigned if the largest party either won or lost a single seat.

To bolster our analysis, we also look at spending. In particular, we look at per capita public spending (both operating and capital expenses). We only have spending data from 1978 and onwards. This spending variable is adjusted using the same benchmarking procedure as the one used for the tax variable. We also use a number of other supporting and control variables in the analysis. Section S1 of the supplementary materials presents an overview of these variables as well as descriptive statistics. All indicators used in the analysis are constructed from publicly available data about the Danish municipalities published by either Statistics Denmark or the Danish Election Database.¹¹

Assessing the Validity of the Design

The key assumption underlying the RD design is that at the cut-off that determines assignment to single party majority status, it was as-if random whether the largest party was actually assigned a majority or not. We cannot test this assumption directly, but we can look for indirect evidence. For one, we can examine whether there are any systematic differences between the type of municipalities where parties narrowly won majorities and the type of municipalities where parties narrowly lost majorities. In section S2 of the supplementary materials, we show that there are no such discontinuities at the assignment cut-off with respect to a number of predetermined variables, which are either closely related to the dependent variable or to the forcing variable.

Another way to probe the as-if randomness assumption is to examine the density of the forcing variable. As such, if we find a large number of observations just below or above the cut-off that assigns single party majority status, this could suggest that local politicians or the electorate were able to decide whether the largest party ended up with a single party majority. However, if one inspects figure 7.3, which plots the density of the forcing variable, there does not seem to be any evidence of this type of sorting. This is supported by the fact that a formal McCrary test of a discontinuity in the density at the cut-off comes out insignificant ($p > 0.8$; see section S3 of the supplementary materials for details). It is not surprising that we find no signs of sorting. As mentioned above, the exact cut-off for assignment of single party majority status is different from election to election, and depends on the exact vote totals of

from 1970 till 2005 is 18, and that in 1980 municipalities sat taxes two percent lower than the average across all years. Then our benchmarked measure of tax rates equals $20 - 18 - (-2) = 4$. As such, the city council in this municipality has implemented a tax level which is 4 percentage points higher than what we would expect from this municipality at this point in time.

¹¹See <http://www.statistikbanken.dk> and <http://valgdata.ps.au.dk>.

the other parties. Even if local politicians or municipal electorates could select into exact vote shares for the largest party, they would be hard pressed to know exactly how many votes this party would need in order to win or lose a majority of the seats in the city council, because the exact number of votes needed would depend on the complete distribution of votes across parties and electoral coalitions, which is, naturally, not revealed before the election.

The Effect of Single Party Majority Status on Tax Policy

We begin our analysis of the relationship between tax rates and single party majority status by plotting benchmarked tax rates against our forcing variable, and by showing the over-all distribution of tax rates for parties who narrowly lost and for parties who narrowly won a single party majority. We plot tax rates at two different points in time. To test the sanctioning hypothesis, which suggested that if the largest party won a majority of the seats in the city council, then the city council would lower taxes before the next election, we plot the tax rate at the election following the one where a party either narrowly won or narrowly lost a majority of the seats in the city council. To test the selection hypothesis, which suggested that having a single party majority incumbent up for election would lead to a city-council that lowered taxes, we plot the tax rate at the election following the one that featured an incumbent party who either narrowly won or narrowly lost a majority of the seats in the city council. In effect, this means that for each close election, we plot the benchmarked election-year tax rates at the time of the next two elections ($t = 1$ and $t = 2$, cf. figure 7.1).

Examining these graphs, we can see that there is no evidence of an immediate effect of single party majority status on tax rates, however, there is evidence of a long term effect. As such, whether or not a party wins a single party majority, does not seem to have an effect on the tax rate set at the next election ($t = 1$), but there does seem to be a negative effect of about half a percentage point if one looks at the subsequent election ($t = 2$). In particular, if we look at the distributions of tax rates, we can see that there are almost no municipalities with high benchmarked tax rates (> 1) among single party majorities at this latter election.

We can interpret these findings as preliminary evidence *against* the sanctioning hypothesis and *for* the selection hypothesis. Accordingly, when the largest party in the city council gets a single party majority, increasing this party's clarity of responsibility for tax policy, there is no evidence that this leads the city council to lower the (benchmarkd) tax rate. This goes against the sanctioning hypothesis,

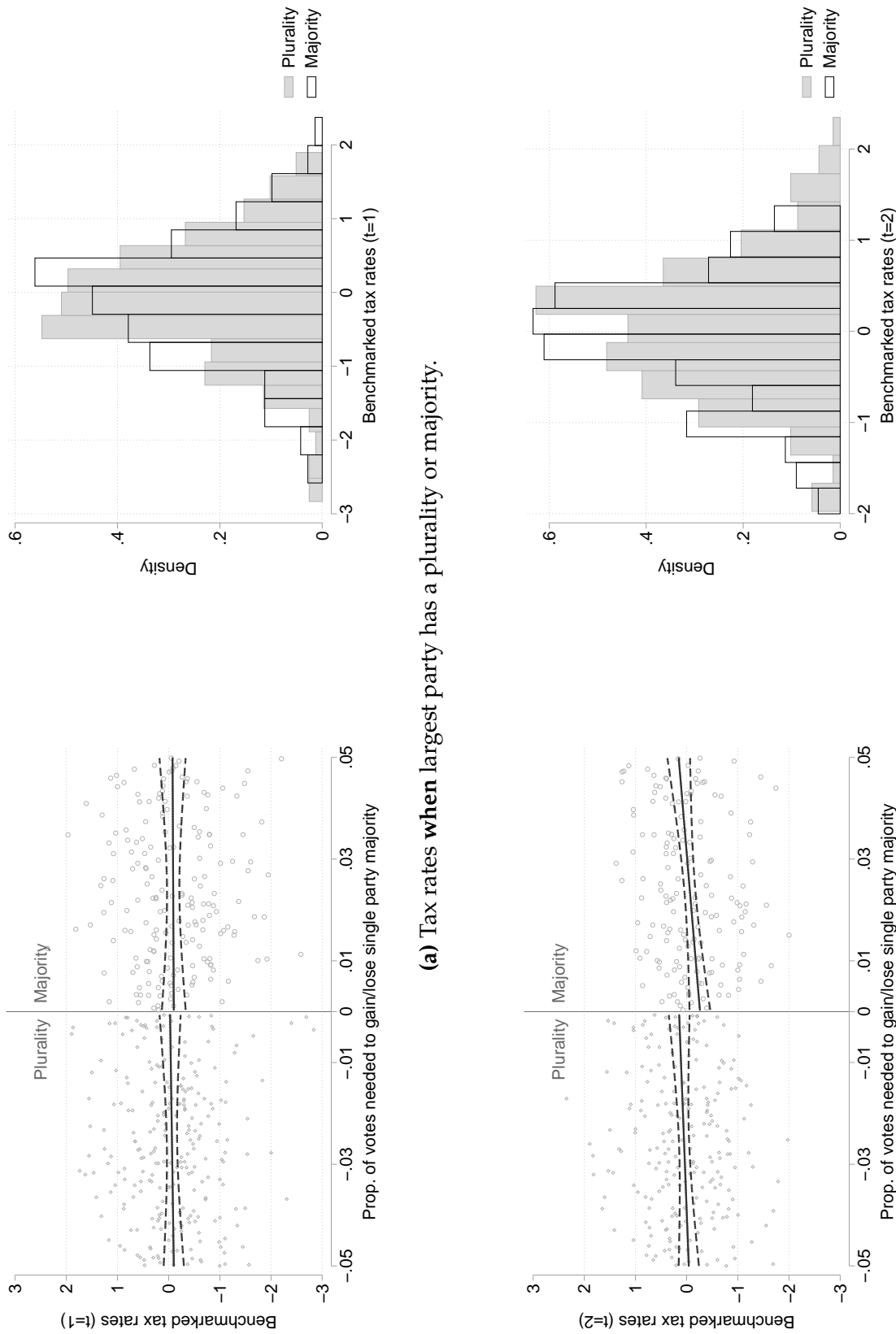


Figure 7.4 How does single party majority status affect tax rates? Left: Relationship between forcing variable and adjusted tax rates for elections in which single party majority status was determined by less than 5 pct. of the vote. Dots are election-year tax levels. Solid lines are expected tax levels estimated as a linear fit of the forcing variable above and below the cut-off. Dashed lines are 95 pct. CIs of this expectation. Right: Histogram of tax rates across single party majority status for elections in which single party majority status was determined by less than 5 pct. of the vote. Top panel: $n=436$. Bottom panel: $n=371$.

which suggested that the prospect of being held more electorally accountable for tax policy by the voters, would lead the largest party to lower taxes before the next election. The evidence does suggest, however, that if the largest party has a majority of the seats going into an election, then the type of city-council that gets elected will be more prone to lower taxes. This is in line with the selection hypothesis, which suggested that if the largest party holds a majority of the seats it is easier for voters to identify and reduce the political influence of parties who are not interested in lowering taxes, and, by extension, they are able to empower parties who are interested in lowering taxes.

Having examined the data graphically, we now try to estimate the effect of single party majority status on tax rates in a statistical model. Specifically, we use the `rdrobust` package developed by Calonico et al. (2014) to do local polynomial point estimation of the effect.¹² We do this using both a first order (local-linear) and a second order (local-quadratic) polynomial. The key estimates from these models are presented in table 7.1.

Table 7.1 Does single party majority status affect tax rates?

Dependent variable	Point estimate	Robust p-value	Bandwidth	Polynomial	n
Tax rates (sanctioning, t=1)	-0.00	0.927	0.05	1	428
Tax rates (sanctioning, t=1)	0.02	0.846	0.08	2	618
Tax rates (selection, t=2)	-0.39	0.016	0.04	1	317
Tax rates (selection, t=2)	-0.42	0.035	0.06	2	478

Estimated using the CCT MSE-optimal bandwidth and a triangular kernel.

As can be seen from table 7.1, the results line up nicely with what we found when examining the data graphically. There is essentially no immediate effect of gaining a single party majority on tax rates, but there is an effect of about -0.4 percentage points on tax rates at the subsequent election. The effect at the subsequent election is substantial: almost half a standard deviation.¹³

To examine the statistical significance of these effects, we can look at the robust p-value associated with the point estimator, which is also reported in table 7.1 (for description of the robust p-value, see Calonico et al., 2014). While the estimated

¹²This entails fitting weighted least squares (WLS) regressions of the forcing variable on tax rates above and below the cut-off, using the difference in model expectations of the WLS regression estimates at the cut-off as the estimator of the effect. To use this method, one needs to choose a bandwidth around the cut-off within in which the WLS models can be fit and a weight structure. Throughout our analysis we follow the advice of Skovron and Titiunik (2015) and use the CCT MSE-optimal bandwidth and a triangular kernel.

¹³In section S11 of the supplementary materials we examine the full time profile and heterogeneity of these effects.

effects on tax rates at $t = 1$ are statistically indistinguishable from zero, the effects on tax levels at $t = 2$ are statistically significant for both the model with a first order polynomial and the model with a second order polynomial ($p < 0.05$).

A potential critique of these significance tests are that they test the effect at $t = 2$ separately from the effect at $t = 1$. That is, there might be a multiple comparisons problem (Gelman, Hill and Yajima, 2012). However, even if one takes the multiple comparisons into account, the statistical evidence for an effect at $t = 2$ is still pretty strong. If one, for instance, makes a simple Benjamini–Hochberg correction of the critical p-values (using an FDR of 0.2), then the estimated effects remain statistically significant.

Taken together, the evidence presented here is consistent with the selection hypothesis, but not with the sanctioning hypothesis. That is, single party majority status does affect tax rates, but it only does so after the single party majority has been up for (re-)election.

Robustness Checks

How model dependent are our results? Following Skovron and Titiunik (2015), we only used data within the CCT MSE-optimal bandwidth of the forcing variable. All decisions about the size of bandwidth, however, rely on a trade-off between bias and efficiency, and therefore it is always interesting to see whether one's results change substantially depending on what trade-off is used. In section S4 of the supplementary materials, we examine whether the results obtained in table 7.1 are sensitive to the size of the bandwidth. We find that the point estimators of the effects are not sensitive to the choice of bandwidth, however, the effect of single party majority status on tax rates at $t = 2$ is statistically insignificant at the smallest bandwidths. This is not surprising, as these include less than a hundred observations.

Another source of model dependence might lie in the local polynomial point estimation. While this method is recommended by political methodologists (Calonico, Cattaneo and Titiunik, 2014; Skovron and Titiunik, 2015), it is still rather new. Accordingly, some might find it interesting to see whether more traditional estimation methods identify similar effects. We look at this in section S5 of the supplementary materials, where we estimate the effect of single party majority status on tax rates using OLS regression. The effects estimated using this method are similar in magnitude and in their statistical significance to the ones that are estimated using local polynomial point estimation.

As a final check of model dependence, we look at whether the local polynomial point estimation *only* identifies an effect on tax levels at the cut-off where single party majority status is assigned. We do this in section S6 of the supplementary materials by examining placebo cut-offs along the forcing variable. There is no evidence of tax levels changing at the alternative cut-offs we examine. We have also looked at whether the results laid out in table 7.1 are sensitive to restricting the observations used, so that all models are estimated using the same observations. This does not affect the results (see section S7 of the supplementary materials for details).

No Evidence of ‘Strategic’ Tax Setting

In sections S8 and S9 of the supplementary materials we examine whether there is any evidence of ‘strategic’ tax setting. That is, we examine whether the election-year decreases in the tax rate identified above (cf. table 7.1) were offset by non-election-year increases in the tax rate, and whether decreases in the income tax rate was offset by increases in other taxes (i.e., property taxes). We find no evidence of this. These analyses tells us that to the extent that changes in tax policy was a result of voters selecting different types of local politicians (cf. the selection hypothesis), voters did not simply select politicians who only seemingly lowered taxes through strategic tax setting (i.e., taking advantage of the election cycle or tax mixing), they elected politicians who, as far as we can tell, earnestly pursued lower taxes.

Alternative Explanations

In section S13 of the supplementary materials, we examine two alternative explanations for why single party majority status might lead to lower taxes, namely, that the effects are a result of increased partisan fragmentation or that they are related to the ideology of the largest party. We find no evidence consistent with these alternative explanations, which lends further credibility to our claim that it is changes in voters mode of selection that is driving the identified changes in tax policy.

Additional Evidence for the Selection Hypothesis

The key mechanism underlying the selection hypotheses is that the way voters select (i.e., vote for) local politicians changes when the largest party gets a single party majority. In particular, the selection hypothesis suggests that electoral support for the largest party becomes more dependent on tax policy when this party controls a

majority of the seats in the city council. So far, we have simply assumed that single party majority status leads to such changes in voter behavior, because that is what the large and empirically powerful literature on clarity of responsibility would predict. In this section, we explore whether this assumption about voters reaction to single party majority status is empirically well-founded.¹⁴

To investigate whether voters hold the largest party more electorally accountable for tax policy when this party controls a majority of the seats in the city council, we estimate four linear regression models that set electoral support for the largest party as a function of our benchmarked income tax measure, a dummy indicating whether the largest party controls a majority of the seats in the city council and an interaction between the two. In addition to this, all models include year fixed effects to control for temporal trends in electoral support. The first two models are lagged dependent variable (LDV) models, with and without a set of political controls meant to index the ideological mood – using a set of dummies for mayoral ideology and the proportion of right-wing voters – as well as the political engagement of the municipal electorate – using proportion who turned out to vote. The second two models are difference-in-difference (DiD) models (and thus include municipality and year fixed effects), with and without the political controls. We use both LDV and DiD models to explore the robustness of our results (as recommended by Angrist and Pischke, 2008, 245).

Figure 7.5 presents the marginal effect of tax rates on support for the largest party for both single party majorities and pluralities derived from these four models. Across all models, the estimated effect of tax rates on support for the largest party is statistically significant and negative when the largest party controls a majority of the seats. Conversely, the effect is close to zero and insignificant if the largest party controls less than a majority of the seats. These differences in effect sizes are statistically significant (cf. section S10 of the supplementary materials).

This tells us that voters are more likely to hold the largest party electorally accountable for the tax rate *if* the party controls a majority of the seats in the city council. What implications does this have? Imagine that the largest party in some municipality narrowly wins a single party majority. If this party is able and willing to lower local income taxes two percentage points, our estimates suggest that the party will be rewarded by the voters with an extra 3-4 percentage points of electoral support at the next election. All else equal, this electoral gain will ensure that the

¹⁴This section builds on our analysis of the electoral consequences of income tax rates described above and detailed in section S14 of the supplementary materials.

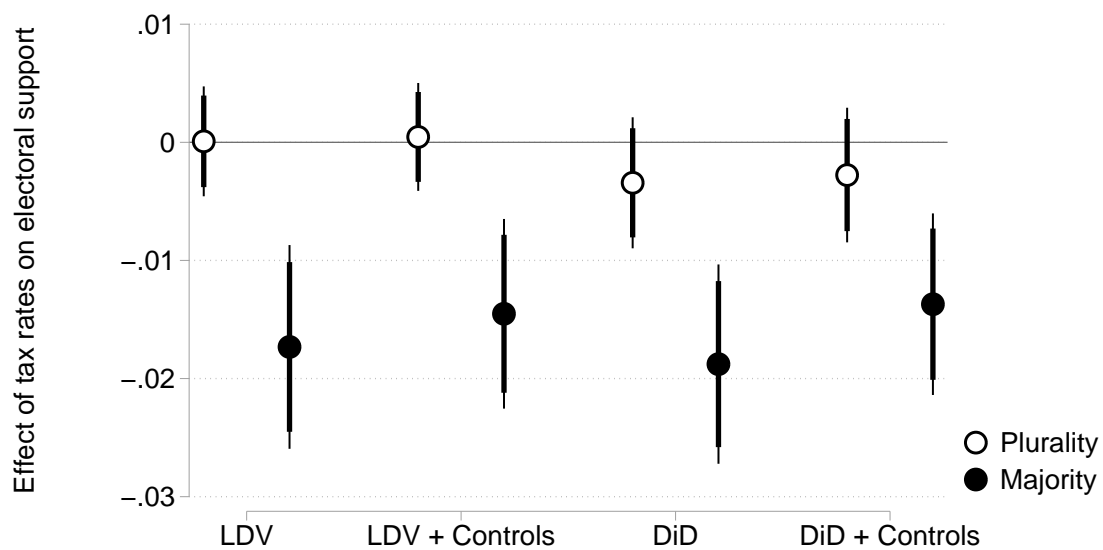


Figure 7.5 Did voters change their mode of selection? Effects of tax rates on electoral support for the largest party in municipalities where the largest party has a plurality or a majority of the seats in the city council. Spikes are 90 (wide) and 95 (thin) pct. confidence intervals. Marginal effects are derived from lagged dependent variable (LDV) and difference-in-difference (DiD) models with and without political controls (turnout, percent right-wing voters and the mayor's ideological orientation). Standard errors clustered at the municipal level. Estimates from these models are presented in section S10 of the supplementary materials.

party can hold onto their majority. If the party is not able or willing to lower local income taxes, but instead raises them, then the party will be punished by the voters, and, all else equal, they will lose their majority. Accordingly, because voters change their mode of selection, single party majority status ends up making the type of parties who lower taxes more powerful and the type of parties who raise taxes less powerful. Arguably, this shift in power will be reflected in the tax policy enacted by the city council, which leaves us with the pattern identified in our main analysis, namely, that single party majority status leads to lower taxes.

A reservation that one might raise with respect to this analysis is that voters are not knowledgeable enough about patterns of political responsibility to act on differences between single party majorities and pluralities (e.g., Achen and Bartels, 2016). There are two reasons why this is not necessarily an important concern in this case. First, single party majority status does seem to have an impact on citizens' basic knowledge about the power structures in the city council. Looking at survey data of a representative cross-section of voters, we find that having a single party majority significantly improves voters ability to correctly name the political party of the mayor and correctly name which party has the most seats in the city council (see section S11 of the supplementary materials). Second, even if voters do not immediately comprehend the changes in political responsibility that single party majority status entails, political elites – such as unions, interest groups and the media – will surely understand what single party majority status means for local politics, and try to communicate this to the electorate.

Conclusion

Clarity of responsibility moderates retrospective voting. Incumbent politicians are more likely to be held electorally accountable for policy outcomes if they are clearly responsible for these outcomes. In this article, we explore whether this has any implications for the type of policy outcomes incumbents pursue in office. Following research on how retrospective voting constrains incumbent behavior, we asserted that clarity of responsibility would increase the extent to which incumbent politicians pursue the type of policy outcomes their electorate prefers.

To examine the viability of this assertion, we looked at tax policy in Danish municipalities. In particular, we examined how an indicator of clarity of responsibility, single party majority status in local city councils, affected the extent to which local officials pursued a policy outcome that was shown to be electorally popular with the

average voter in the Danish municipalities: lower income taxes. Using an RD design, we were able to identify the causal effect of single party majority status on tax policy by comparing municipalities where the largest party narrowly obtained an single party majority with municipalities where the largest party fell just short of such a majority.

We found that single party majority status did lead to lower taxes, however, the effect was not immediate. When the largest party in the city council got a single party majority, increasing its clarity of responsibility for tax policy, it did not lower or refrain from raising taxes. Instead, our analyses identified an effect at the subsequent election. That is, the type of city-council elected when a single party majority was up for re-election tended to lower taxes.

We attributed this effect to voters changing their mode of selecting local politicians when a single party majority was part of their set of electoral choices. In particular, we showed that if the largest party won a single party majority, they were held more electorally accountable for the tax rate the city council enacted. Arguably, this change in voters mode of selection meant that single party majority status was eventually accompanied by a transfer of seats in the city council from the type of parties who raise taxes and towards the type of parties who lower taxes, and this transfer of seats lead some city councils to adopt lower taxes.

In summary, we found that clarity of political responsibility had a ‘selection’ rather than a ‘sanctioning’ effect on incumbent behavior. That is, while increases in clarity of responsibility did mean that incumbents were sanctioned more harshly for enacting unpopular tax policy, this change in re-election incentives did not make the incumbent pursue different policies. Rather, clarity of responsibility affected incumbent behavior by affecting who got to stay in power and who did not. As a result, the local politicians elected when clarity of responsibility was high were more likely to pursue the more popular policy of lowering taxes.

It is surprising that we do not find any evidence of a sanctioning effect. Much previous literature on how elections discipline incumbents have found evidence of sanctioning effects (e.g., Alt, De Mesquita and Rose, 2011; Besley and Case, 1995a). Even so, we are not the first to find that selection dominates sanctioning when it comes to the effects of clarity of responsibility (e.g., Duch and Stevenson, 2008). In any case, our findings highlight the important role electoral selection plays when it comes to electoral control of the policies political leaders pursue.

How generalizable are our findings? Since we used an RD design, we can only generalize directly to the types of municipalities where the largest party was close to

either winning or losing a single party majority (Lee, 2008). Fortunately, this is not just a small number of municipalities, because the exact cut-off for assignment of single party majority status varies substantially across municipalities and elections. In fact, 235 (out of 273) municipalities appear in our dataset of close elections at least once. Therefore, we believe that the effects identified above can be generalized to practically all Danish municipalities. The more interesting scope of inference, however, is political systems in general. It is incredibly hard to say whether these findings will generalize to other contexts, however, it is important to note that we have found evidence of the importance of clarity of responsibility in a context with a strong consensual culture and with a committee system which forces parties to work together – even if a single party controls a majority of the seats (Serritzlew, Skjæveland and Blom-Hansen, 2008). In other contexts, where differences in clarity of responsibility are more dramatic, the effects might also be more dramatic.

Turning to implications, the most important one seems to be that incumbents will do more to provide the policy outcomes that the average voter prefers when clarity of responsibility is high. To the extent that democratic quality can be measured in terms of responsiveness to the average voter (e.g., Lax and Phillips, 2012), this suggests that a certain degree of clarity of responsibility is a desirable feature of a political system. However, it is important to remember, that responsiveness to the average voter is not only a force for good. The electorate may deviate from the population at large, twisting the incentives of politicians away from the people they are meant to serve (Sances, 2016), or the preferences of the electorate may be ill informed, leading to bad decision-making if politicians are forced to follow them too closely (Bartels, 2005). Regardless of whether responsiveness is a force for good or not, our findings suggests that in a representative democracy, clarity of political responsibility, and the centralization of power which it entails, does carry an apparent benefit to an electorate that wants to steer the action of its political leaders: it means that it will be easier for the electorate to identify what type of political leaders they have at the present, and easier to retain the political leaders which are actually willing and able to act in their interest.

Supplementary materials

S1: Descriptive Statistics

Tables S.7.1 and S.7.2 report descriptive statistics on the variables used in the analysis for the close elections and for all elections. Table S.7.3 presents a description of the same variables.

Table S.7.1 Descriptive statistics (close elections)

	Mean	SD	Min	Max	n
Election year	1985.62	10.13	1970.00	2001.00	839
Size of city council	16.52	3.27	9.00	29.00	839
Single party majority	0.39	0.49	0.00	1.00	839
Population (log)	9.33	0.74	7.90	12.12	839
Area (log)	4.70	0.80	2.17	6.29	839
Right-wing voters	0.49	0.17	0.00	0.84	839
Turnout	0.75	0.06	0.55	0.95	839
How close was the election? (seats)	-0.61	0.49	-1.00	0.00	839
How close was election? (votes/10)	-6.65	61.65	-338.00	446.00	839
Forcing variable	-0.02	0.07	-0.21	0.20	839
Tax level (unadjusted), t=0	18.00	2.64	9.40	23.20	741
Per capita spending (unadjusted)	30.19	10.33	11.76	65.34	595
Support for the largest party, t=0	0.45	0.06	0.27	0.91	839
Election cycle	5.07	2.63	1.00	9.00	839
Forcing variable, absolute values	0.06	0.04	0.00	0.21	839
Tax level, t=0	-0.02	0.85	-3.76	3.53	741
Tax level, t=1	-0.04	0.83	-3.76	2.37	834
Tax level, t=2	-0.00	0.75	-2.32	2.35	731
Tax level, t=3	0.05	0.70	-2.00	2.84	632
Tax level, t=4	0.08	0.67	-1.97	2.84	535
Property tax, t=1	0.05	2.62	-14.00	22.53	642
Property tax, t=2	-0.12	2.49	-14.00	15.02	636
Per capita spending, t=1	0.14	4.57	-8.70	23.72	593
Per capita spending, t=2	0.28	4.65	-8.38	23.64	492
Average tax level, t=1	-0.04	0.79	-3.38	2.86	834
Average tax level, t=2	0.00	0.71	-1.97	2.88	731
Right-wing mayor	0.64	0.61	0.00	2.00	839
Retain SPM	0.30	0.46	0.00	1.00	839
Support for the largest party, t=1	0.43	0.08	0.21	0.89	739

Figure S.7.1 shows the development of the tax rates for all municipalities in the time period under investigation. Interestingly, we can see that taxes were rising sharply in the 1970s and 1980s. In this period the average tax rate rose from around 14 percent to about 20 percent. The primary explanation for this uniform increase in taxes in this period is the expansion of the Danish welfare state, which left the municipalities with a lot of new tasks that needed to be financed. This might also

Table S.7.2 Descriptive statistics (all elections)

	Mean	SD	Min	Max	n
Election year	1987.38	11.06	1970.00	2005.00	2841
Size of city council	17.16	4.13	9.00	55.00	2475
Single party majority	0.24	0.43	0.00	1.00	2475
Population (log)	9.37	0.79	7.73	13.35	2475
Area (log)	4.81	0.77	2.17	6.33	2475
Right-wing voters	0.48	0.17	0.00	0.84	2475
Turnout	0.75	0.06	0.47	0.95	2475
How close was the election? (seats)	-1.61	1.67	-12.00	4.00	2475
How close was election? (votes/10)	-6.65	61.65	-338.00	446.00	839
Forcing variable	-0.02	0.07	-0.21	0.20	839
Tax level (unadjusted), t=0	18.38	2.67	9.40	23.20	2452
Per capita spending (unadjusted)	29.64	10.52	11.03	79.40	1813
Support for the largest party, t=0	0.42	0.11	0.21	1.00	2473
Election cycle	5.04	2.57	1.00	9.00	2559
Forcing variable, absolute values	0.06	0.04	0.00	0.21	839
Tax level, t=0	-0.00	0.84	-3.94	3.53	2452
Tax level, t=1	-0.00	0.84	-3.94	3.53	2452
Tax level, t=2	0.00	0.77	-3.63	3.28	2179
Tax level, t=3	0.00	0.72	-3.34	2.84	1906
Tax level, t=4	0.00	0.69	-2.54	2.91	1633
Property tax, t=1	0.00	2.50	-14.00	22.53	1911
Property tax, t=2	0.00	2.42	-14.00	22.53	1911
Per capita spending, t=1	-0.00	4.59	-9.54	37.35	1813
Per capita spending, t=2	-0.00	4.80	-9.54	37.35	1554
Average tax level, t=1	0.00	0.80	-3.73	2.88	2452
Average tax level, t=2	0.00	0.74	-3.73	2.88	2179
Right-wing mayor	0.92	0.73	0.00	2.00	2841
Retain SPM	0.17	0.38	0.00	1.00	2841
Support for the largest party, t=1	0.41	0.10	0.21	0.92	2197

Table S.7.3 Variable description

Variable name	Description
Election Year	Year before election period started.
Election Cycle	Election Cycle examined (1=1971-1974, 2=1975-1978 and so on).
Size of city council	Number of seats.
Single party majority	Dummy indicating whether the largest party controls a majority of the seats in the city council.
Population (log)	Logged number of inhabitants in the beginning of the election year.
Area (log)	Logged number square kilometers the municipality covers.
Right-wing voters	Proportion of voters for the Conservative or the Liberal party (Venstre).
Turnout	Proportion who turned out in the election.
How close was the election? (seats)	Number of seats largest party would have needed to obtain/lose a single party majority.
How close was the election? (votes/10)	Number votes largest party would have needed to win/lose a single party majority divided by 10. Only coded for close elections.
Forcing variable	Proportion of votes largest party would have needed to win/lose a single party majority. Only coded for close elections.
Tax level (unadjusted)	The municipalities income tax rate.
Tax level	The municipalities benchmarked income tax rate. Measured in the election year where single party majority is assigned (t=0) and at the next four elections (t=1,2,3,4).
Average tax level	The average benchmarked income tax rate across the entire election period. Measured in the election period where single party majority is assigned (t=1) and in the next election period (t=2).
Per capita spending (unadjusted)/1000 DKK	The municipalities public spending per capita in 1000 DKK. Includes all operating and capital expenditures.
Per capita spending/1000 DKK	The municipalities benchmarked public spending per capita in 1000 DKK. Includes all operating and capital expenditures. Measured in the election year after single party majority is assigned (t=0) and in the subsequent election (t=2).
Property tax level	The municipalities benchmarked property tax rate in 10th of a percent. Measured in the election year after single party majority is assigned (t=0) and in the subsequent election (t=2).
Support for the largest party	Proportion who votes for the the largest party in the municipality. Measured in the election where single party majority was assigned and in the next election (t=0,1).
Retain SPM	Dummy variable indicating whether single party majority is retained.
Right-wing mayor	Categorical variable indicating whether mayor is left wing (0, the Social Democratic or Socialist Peoples party), right-wing (1, the Conservative party or the Liberal party) or non-ideological (3, other parties) .

explain why the average voter preferred lower taxes, as the differences in tax rates did not simply translate into different levels of public goods provision, but also indicated the extent to which government mandated tasks were carried out efficiently.

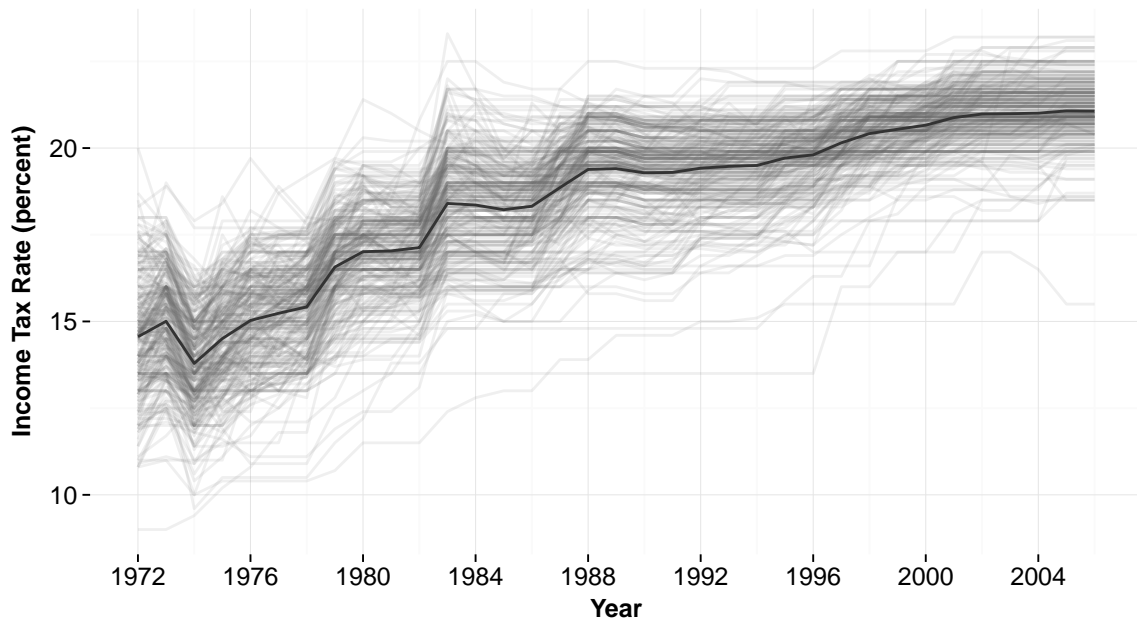


Figure S.7.1 Local income tax levels from the Danish national statistics bureau (Statistics Denmark) on the 273 Danish municipalities between 1972 and 2006. The black line indicates the average for all municipalities. The grey lines show tax rates for individual municipalities.

Figure S.7.2 plots the share of single party majorities across all municipalities for all elections. 24 percent of all elections result in a single party majority. Across time this share varies from 15 percent in the 1985 election to 35 percent in the 2001 election.

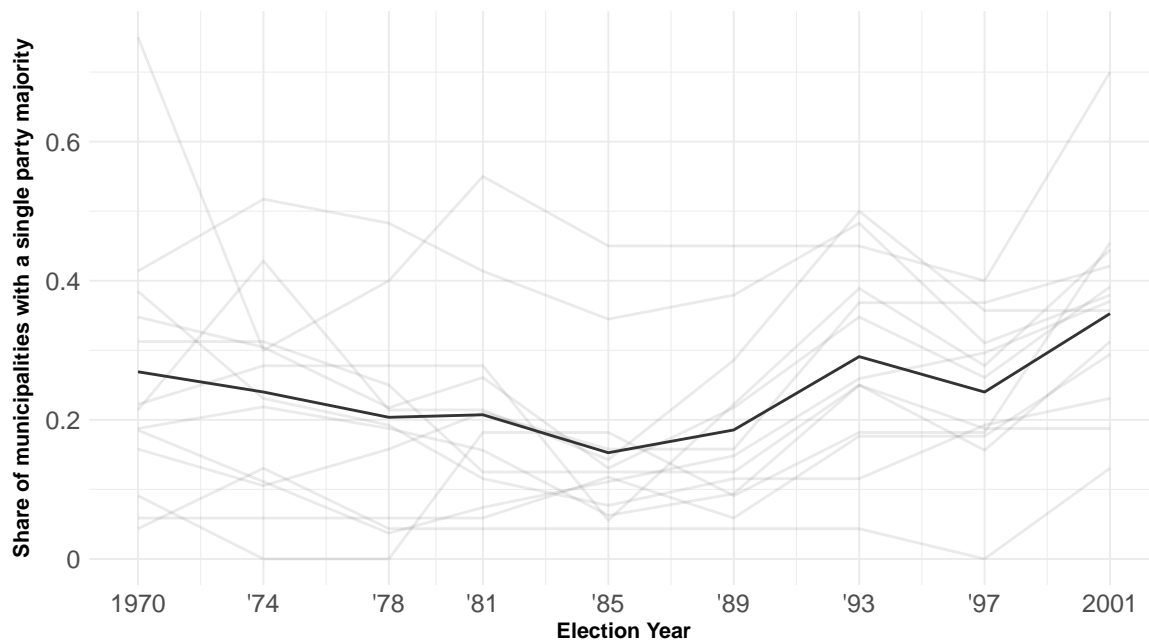


Figure S.7.2 The share of municipalities with a single party majority across all elections (1970 to 2001). The black line indicates the mean across all municipalities. The grey lines are the shares for each of the 14 Danish regions.

S2: Discontinuity in predetermined variables

In table S.7.4 we use local polynomial point estimation to estimate whether there is an identifiable discontinuity around assignment to single party majority status on a set of pre-treatment variables (i.e. variables determined before single party majority status was assigned). The variables we look at are either lagged versions of the dependent variables we look at (i.e., tax levels and spending), related to the forcing variable (support for the largest party, council size) or related to the political make-up of the municipality (ideology og voters/mayor and turnout.)

We use the CCT MSE-optimal bandwidth, a triangular kernel and a local-linear specification. If we can identify an discontinuity for these pre-treatment variables this means that it is possible for certain parties to select into single party majority status at the cut-off, threatening the identifying assumption of no sorting. To take into account, that we are examining discontinuities for multiple variables, we also display Benjamini-Hochberg critical values.

As can be seen from table S.7.4, there are no signs of a statistically significant discontinuity around the cut-off for any of the pre-treatment variables we examine (i.e., no p-value falls below the B-H critical value).

Table S.7.4 Is there a discontinuity in predetermined variables?

Variable	Point Estimator	p-value	B-H critical value	Bandwidth	n
Per capita spending (unadjusted)	3.31	0.28	0.14	0.04	269
Tax level (unadjusted), t=0	0.66	0.24	0.11	0.04	336
Support for the largest party, t=0	0.00	0.83	0.25	0.06	499
Size of city council	0.08	0.75	0.21	0.04	349
Right-wing voters	-0.02	0.65	0.18	0.05	471
Turnout	-0.02	0.15	0.07	0.04	357
Right-wing mayor	-0.23	0.06	0.04	0.05	432

Estimated using the CCT MSE-optimal bandwidth, a triangular kernel, and a local-linear specification. Benjamini-Hochberg critical values calculated using 0.25 FDR.

S3: McCrary Test

There was no evidence of sorting, cf. the McCrary test reported in figure S.7.3.

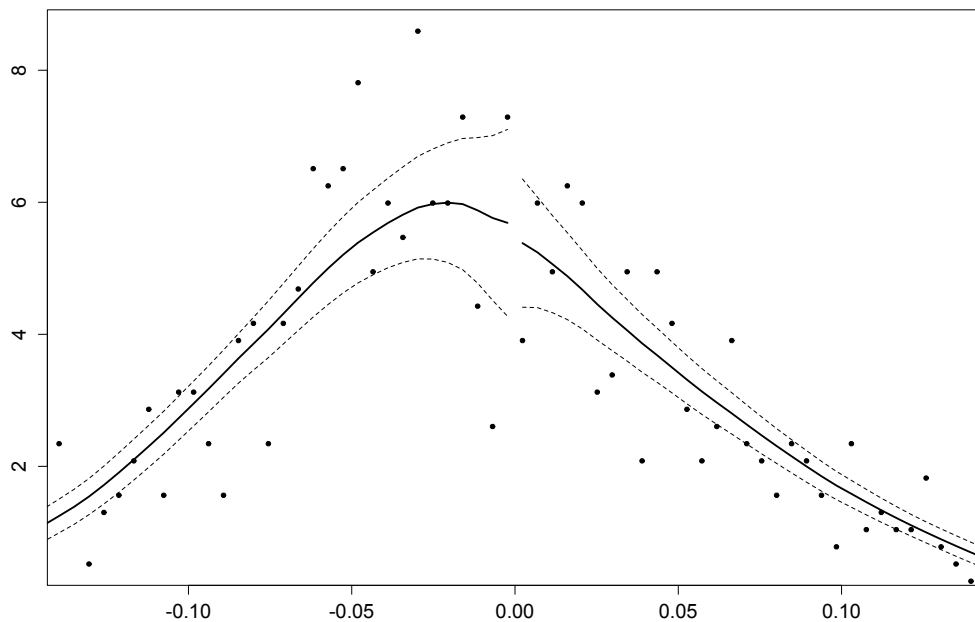


Figure S.7.3 Is there a discontinuity in the density of the forcing variable around the cut-off? McCrary test. Plot produced using package `rdd` in R. There is no evidence of sorting around the cut-off ($p \approx 0.8$).

S4: Sensitivity to Bandwidth Choice

In figure S.7.4 we look at whether the effect-estimates vary depending on what bandwidth is chosen. The figure reveals little variation in the effect sizes, suggesting that the results are not sensitive to choice of bandwidth.

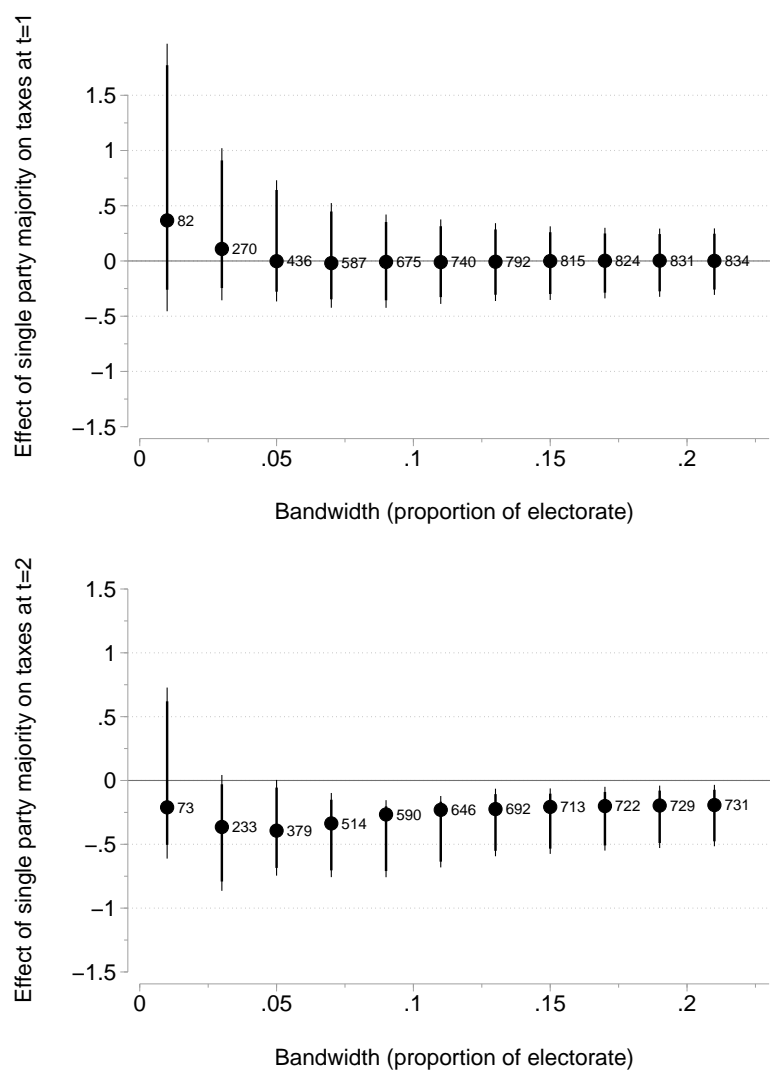


Figure S.7.4 How sensitive are the results to changes in bandwidth? Dots signify estimated effects of single party majority status on tax rates with different bandwidths along with 90 pct. (thick) and 95 pct. (thin) confidence intervals. The effects and their variance were estimated using local polynomial point estimation with a triangular kernel. The numbers next to the dots are the number of observations within the particular bandwidth.

S5: Alternative Estimation Methods

Below we estimate the effect of single party majority status on tax rates using an OLS regression. We estimate six linear models predicting tax rates at $t = 1$ and at $t = 2$ (cf. figure 7.1). We use a standard OLS regression with robust and municipality-clustered standard errors to estimate the models. In the first model we include a single party majority dummy and our forcing variable, in the second model we also include a quadratic version of our forcing variable, and in the third model we substitute the quadratic term for an interaction between the forcing variable and the single party majority dummy. In the fourth, fifth and sixth models we include a small battery of controls and otherwise run the same specifications as in models one through three. The controls used are: population size (log), size of municipality (log), size of the city council (seats), support for right wing parties in the close election (pct.), turnout in the close election (pct.) and the ideology of the mayor's party (right-wing, left-wing or non-ideological). The key estimates from these models are presented in tables S.7.5 and S.7.6.

As can be seen from tables S.7.5 and S.7.6 the estimated effects are very close to the ones arrived at when using the local polynomial point estimation (cf. table 7.1).

Table S.7.5 Effect of single party majority status on on tax rates at $t=1$ (sanctioning)

	(1)	(2)	(3)	(4)	(5)	(6)
Single party majority	-0.08 (0.17)	-0.07 (0.17)	-0.07 (0.16)	-0.13 (0.16)	-0.13 (0.16)	-0.13 (0.16)
Forcing variable	1.05 (2.83)	0.93 (2.85)	1.47 (3.93)	1.88 (2.77)	1.74 (2.77)	2.29 (3.90)
Forcing variable \times Forcing variable		-16.01 (52.31)			-18.42 (52.65)	
Single party majority \times Forcing variable			-0.98 (5.63)			-0.97 (5.63)
Controls				✓	✓	✓
Observations	436	436	436	436	436	436
R squared	0.00	0.00	0.00	0.04	0.04	0.04
RMSE	0.82	0.82	0.82	0.81	0.81	0.81

Standard errors in parentheses

Estimated using a 5 pct. bandwidth.

Controls: Population, area, turnout, percent right-wing voters, mayoral party.

⁺ $p < 0.1$, * $p < 0.05$

Table S.7.6 Effect of single party majority status on on tax rates at $t=2$ (selection)

	(1)	(2)	(3)	(4)	(5)	(6)
Single party majority	-0.40* (0.14)	-0.41* (0.14)	-0.41* (0.14)	-0.44* (0.14)	-0.45* (0.14)	-0.44* (0.14)
Forcing variable	5.75* (2.43)	6.08* (2.45)	3.79 (3.27)	6.24* (2.46)	6.48* (2.49)	4.75 (3.34)
Forcing variable \times Forcing variable		48.93 (48.81)			38.28 (50.40)	
Single party majority \times Forcing variable			4.55 (4.89)			3.42 (5.09)
Controls				✓	✓	✓
Observations	379	379	379	379	379	379
R squared	0.02	0.02	0.02	0.04	0.04	0.04
RMSE	0.72	0.72	0.72	0.72	0.72	0.72

Standard errors in parentheses

Estimated using a 5 pct. bandwidth.

Controls: Population, area, turnout, percent right-wing voters, mayoral party.

+ $p < 0.1$, * $p < 0.05$

S6: Placebo Test

In figure S.7.5 we use local polynomial point estimation to estimate the effect of being above or below a number of so-called placebo cut-offs. That is, the effect of being above or below a cut-off at which nothing happens. We estimate the effect at these cut-off for taxes at $t = 1$ and $t = 2$. In our estimation we use the CCT MSE-optimal bandwidth, a triangular kernel and a local-linear specification.

We do not identify a statistically significant effect at any of the placebo cut-offs.

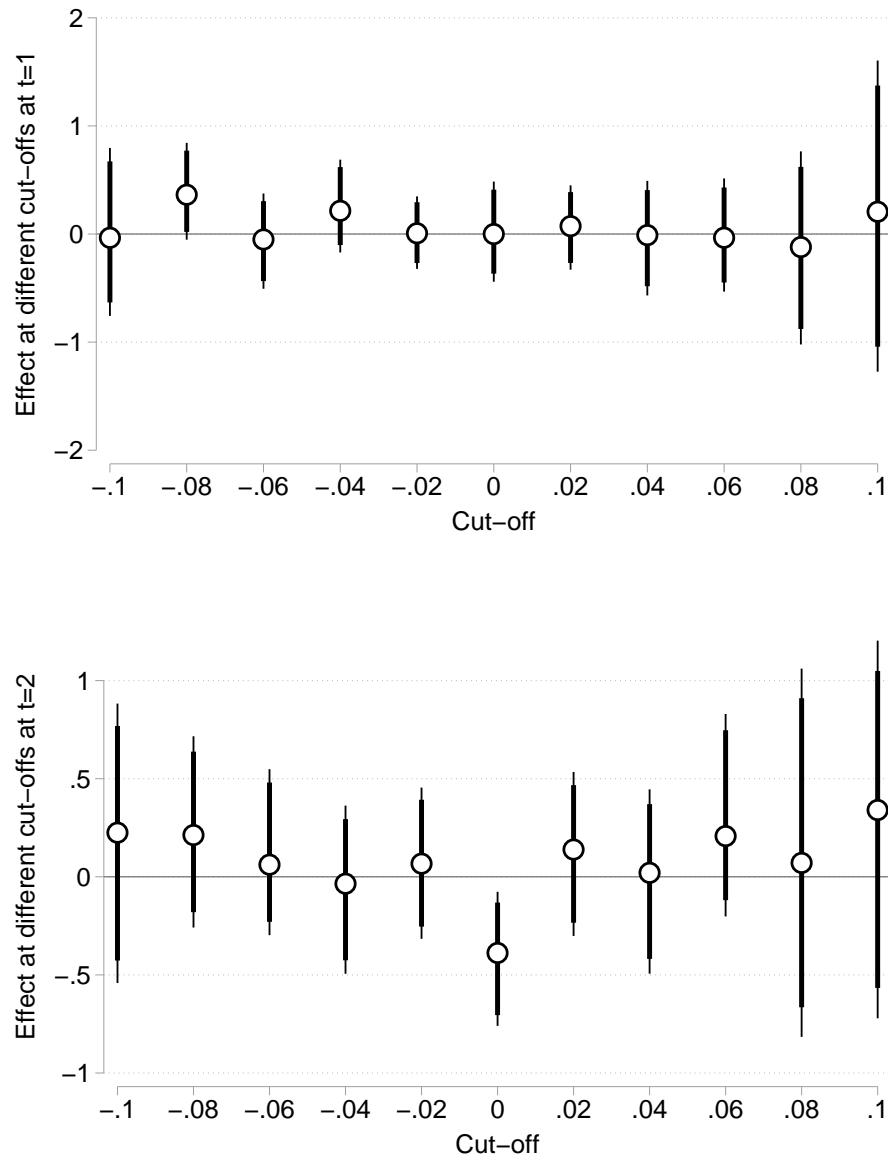


Figure S.7.5 Effects at placebo cut-offs and the actual cut-off (0) which assigned single party majority status. Effects estimated using local polynomial point estimation.

S7: Balanced Panel

In table S.7.8 we replicate table 7.1 excluding the year 2001, which was not included for the original analyses of taxes at $t = 2$, because we do not have tax levels for the election in 2009 (see the section ‘Income Taxes in Danish Municipalities’ for an explanation of why we do not have tax rates in 2009). This does not change the results.

Table S.7.7 Does single party majority status affect property tax rates? (excluding 2001)

Variable	Point Estimate	p-value	Bandwidth	Polynomial	n
Tax rates (sanctioning, t=1)	-0.06	0.841	0.05	1	388
Tax rates (sanctioning, t=1)	0.00	0.870	0.07	2	508
Tax rates (selection, t=2)	-0.39	0.016	0.04	1	317
Tax rates (selection, t=2)	-0.42	0.035	0.06	2	478

Estimated using the CCT MSE-optimal bandwidth and a triangular kernel.

S8: Examining the Election Period Average Tax Rate

Above we only examined the tax rate in the election year, however, if single party majority status actually affected the city councils' propensity to pursue lower taxes, then we should be able to find this effect in the entire election period, not just in the election year. In particular, we want to make sure that local politicians do not offset lower taxes in an election year by raising taxes in the beginning of the election period. In table S.7.8 we replicate table 7.1 using the average tax rate across the entire election period. The results are pretty much the same as what we found in table 7.1. One difference is that the effects estimated at $t = 2$ seems to be slightly smaller. As such, there seems to be a larger negative effect in election years, however, this tendency is not very strong. In particular, the absolute difference between the estimates for the election year tax rate and the average tax rate is less than 0.1.

Table S.7.8 Does single party majority status affect average tax rates?

Variable	Point estimate	p-value	Bandwidth	Polynomial	n
Tax rates (sanctioning, t=1)	-0.06	0.701	0.05	1	474
Tax rates (sanctioning, t=1)	-0.06	0.817	0.08	2	627
Tax rates (selection, t=2)	-0.32	0.038	0.05	1	356
Tax rates (selection, t=2)	-0.34	0.102	0.07	2	504

Estimated using the CCT MSE-optimal bandwidth and a triangular kernel.

S9: Examining Property Taxes

Municipalities in Denmark can also levy a property tax. There are several reasons why we did not use the property tax in the main analysis: they are not as salient, they constitute a much smaller fraction of the municipalities' income, they are only directly paid by those who own their own home, and it is not clear that there is an electoral incentive to lower them (Blom-Hansen, Monkerud and Sørensen, 2006). However, in spite of this, it is interesting to briefly examine the property tax. As such, by examining property tax rates it becomes possible to find out whether single party majority status does in fact lead to a lower over-all tax level, or whether the tax mix simply changes from income to property taxes.

In table S.7.9 we replicate table 7.1 using property taxes as the dependent variable. The property taxes are measured in tenths of a percent. As can be seen from this table, we find no evidence of tax-mixing, if anything, property taxes seem to drop as well in $t = 2$, however, the estimated effects of single party majority status on property taxes are not statistically significant.

Table S.7.9 Does single party majority status affect property tax rates?

Dependent Variable	Point Estimate	Robust p-value	Bandwidth	Polynomial	n
Tax rates (sanctioning, t=1)	-0.04	0.942	0.06	1	384
Tax rates (sanctioning, t=1)	0.40	0.528	0.06	2	414
Tax rates (selection, t=2)	-0.55	0.336	0.06	1	400
Tax rates (selection, t=2)	-0.29	0.807	0.06	2	400

Estimated using the CCT MSE-optimal bandwidth and a triangular kernel.

S10: Full Model

Table S.7.10 presents the key estimates from the regression models underlying figure 7.5.

Table S.7.10 OLS regression of largest party vote share

	(1)	(2)	(3)	(4)
Single party majority	0.017* (0.006)	0.023* (0.006)	0.026* (0.006)	0.028* (0.006)
Tax level, $t=1 \times$ Single party majority	-0.014* (0.005)	-0.015* (0.005)	-0.015* (0.005)	-0.015* (0.005)
Support for the largest party, $t=0$	0.562* (0.037)	0.496* (0.035)		
Political Controls		✓		✓
Year FE	✓	✓	✓	✓
Municipal FE			✓	✓
Observations	2180	2180	2182	2182

Municipality Clustered Standard errors in parenthesis.

Political controls: Turnout, percent right-wing voters, mayoral party ideology.

S11: Citizen Survey on Local Politics (1978)

In this section, we examine responses from a survey conducted in 1978 on a representative sample of citizens from 70 municipalities ($n = 1,856$).¹⁵ The survey was conducted in the month following the 1978 municipal election. Respondents were asked about the political party of the mayor and the largest political party in the council. 88 pct. could (correctly) name the political party of the mayor while 71 pct. could name the largest political party in the council. In table S.7.11 we test whether the share of correct responses differ among municipalities with and without a single party majority. On both dimensions, we see that citizens in municipalities with a single party majority are significantly more likely to provide a correct response. This is particular true for naming the largest political party, which improves by 10 percentage points when there is a single party majority.

Table S.7.11 OLS regressions of citizens' knowledge about local politics (1978 survey)

	Can the respondent correctly name...	
	...The party of the mayor?	...The largest party in the council?
Single party majority	0.04* (0.02)	0.10* (0.02)
Constant	0.87* (0.01)	0.68* (0.01)
Observations	1,856	1,856
Adjusted R ²	0.002	0.01

Standard errors in parentheses. * $p < .05$

¹⁵Data from this survey was acquired through the Danish Data Archive, see <http://dda.dk/catalogue/199?lang=da> for details.

S12: Heterogeneity in the Effects

Our selection and sanctioning hypotheses only made predictions about tax levels at $t = 1$ and $t = 2$, but what is the complete temporal profile of the single party majority effect? We explore this in figure S.7.6, estimating the effect of single party majority on election-year tax rates at the four elections following assignment of single party majority. Here we can see that the effect does not kick in until $t = 2$, as we found in our analysis above, and then the effect dissipates at $t = 3$ and $t = 4$.

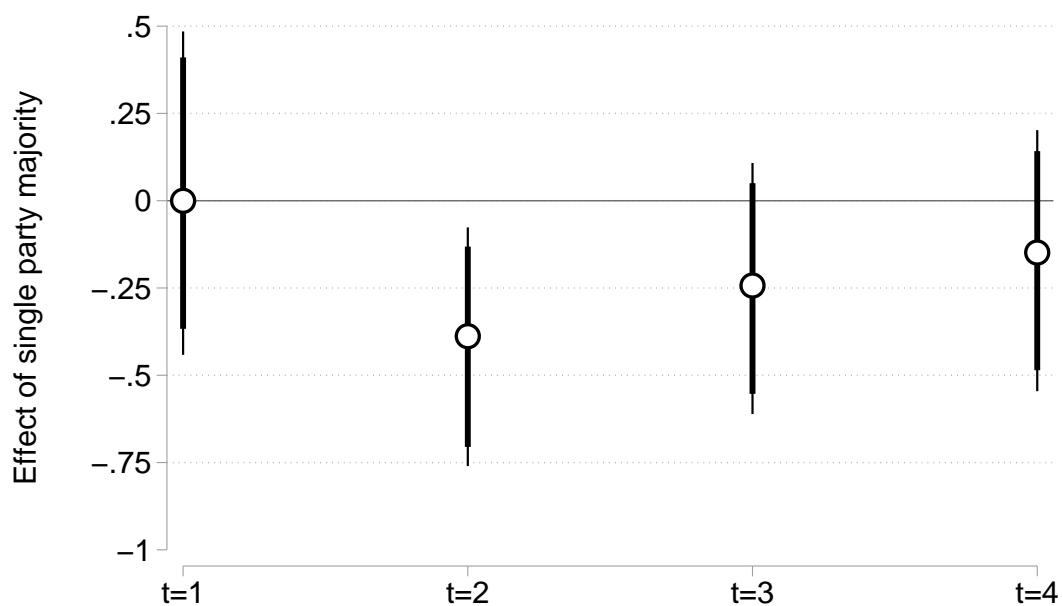


Figure S.7.6 Effect of obtaining a single party majority on benchmarked tax levels at $t=1, 2, 3$, and 4 . Spikes are 90 pct. (thick) and 95 pct. (thin) confidence intervals. Effects estimated using local polynomial point estimation with a first order polynomial, a triangular kernel and the CCT MSE-optimal bandwidth.

In addition to this temporal profile of the effect, we can look at whether the effect is larger in some municipalities than it is in others. In table S.7.12 we examine whether the effect of single party majority status on property tax rates at $t = 2$ depends on whether the tax rate before the close election was above or below the median tax rate (18.5), whether right wing parties received support from more or less than 50 pct. of voters in the close election, and whether the election took place before or after 1984.

These analyses show that the effect is stable across the time period investigated. More importantly, the effect does not seem to depend on the ideological make-up of

Table S.7.12 Does single party majority status affect property tax rates at $t=2$?

Analysed group	Point Estimate	Robust p-value	Bandwidth	Polynomial	n
Below median tax rate	-0.25	0.374	0.04	1	162
Above median tax rate	-0.59	0.006	0.04	1	156
Support for right-wing parties < 50 pct	-0.40	0.064	0.04	1	168
Support for right-wing parties > 50 pct	-0.39	0.094	0.05	1	154
Before 1984	-0.34	0.115	0.05	1	188
After 1984	-0.43	0.070	0.04	1	140

Estimated using the CCT MSE-optimal bandwidth and a triangular kernel.

the electorate. The effects on tax rates in $t = 2$ are the same in municipalities where a majority of the voters support right-wing parties. This suggests, that our results do not hinge upon the ideological make-up of the Danish electorate. We do find, however, that the effect depends on the over-all tax rate in the municipality. If the municipality starts out with a tax rate that was above the median, then the effect of single party majority status on tax rates at $t = 2$ tends to be higher.

S13: Alternative Explanations

Gaining single party majority status might have effects on other things than just the mode of selection adopted by the voters, and these things could potentially affect tax setting as well. For one, single party majority status reduces partisan fragmentation in the city council and reduces the number of parties who have a say in municipal policy-making. If one conceptualizes public spending as a common pool resource, then political economy models tell us that as fragmentation decreases so does public spending (Bawn and Rosenbluth, 2006; Martin and Vanberg, 2013; Velasco, 2000; Weingast, Shepsle and Johnsen, 1981). To the extent that this lower spending translates into lower taxes as well, this might explain our findings. However, existing studies on the relationship between party fragmentation and spending in Danish municipalities have gotten mixed results (Blom-Hansen, 2010; Hansen, 2014). Further, if the decrease in party fragmentation and need for inter-party cooperation is what is driving down taxes, it is not clear why this effect does not appear immediately after the election of a single party majority (i.e., why is there no effect at $t = 1$?).

Even so, it is still interesting to explore whether single party majority status affects the level of municipal public spending. We estimate the effect of single party majority status on per capita spending using the same local polynomial point estimation method we used for examining effects on income taxes. We estimate effects at both $t = 1$ and $t = 2$ with both a local-linear and a local-quadratic fit. The key estimates from these models are presented in table S.7.13. As can be seen from this table, there is a positive point estimate of the effect on spending across both time periods, but the effect is not statistically distinguishable from zero. Note that this goes against the notion that single party majority status should affect tax rates through decreased spending. As such, it does not seem to be the case that municipalities lower taxes because they lower public spending. Instead, municipalities must be identifying other ways of getting additional revenue, while maintaining the same level of spending (e.g., increase their debt or increase other sources of revenue).

Table S.7.13 Does single party majority status affect per capita spending?

Dependent variable	Point estimate	Robust p-value	Bandwidth	Polynomial	n
Spending (sanctioning, $t=1$)	0.79	0.462	0.05	1	280
Spending (sanctioning, $t=2$)	1.19	0.412	0.07	2	402
Spending (selection, $t=1$)	0.34	0.751	0.05	1	238
Spending (selection, $t=2$)	0.61	0.682	0.06	2	325

Estimated using the CCT MSE-optimal bandwidth and a triangular kernel.

Spending is benchmarked and measured in 1,000 DKK per capita (\approx 150 USD).

Another alternative explanation is that the effects identified above are related to the ideology of the largest party. In particular, if right-wing parties are more interested in lowering taxes than left-wing parties, and if the largest party is more often right-wing than left-wing, then this might explain why taxes go down when the largest party wins a majority of the seats in the city council (Lowry, Alt and Ferree, 1998).

In fact, the largest party in the Danish municipalities is more often right-wing than left-wing. 45 pct. of the largest parties are right-wing, 35 pct. are left-wing and the last 20 pct. are parties who do not lean strongly left or right. However, if this ideological imbalance is what is driving down tax levels, it is not clear why single party majority status has no immediate effect on tax rates (i.e., no effect at $t = 1$). Further, if we analyze our data separately for cases where the largest party is right-wing and for cases where the largest party is left-wing, we find no substantial differences in the results, cf. figure S.7.7. That is, the tax policy pursued by the city council when the largest party wins a majority of the seats, does not depend on the largest party's ideological orientation.

In sum, there is no evidence that the effect of single party majority status on tax policy is based on changes in party fragmentation, coalition structure or the ideological composition of the largest parties. This, combined with the evidence for the key mechanism underlying the selection hypothesis, lends credibility to the fact that it is changes in voters mode of selection that is driving the changes in tax policy we identified in our main analysis.

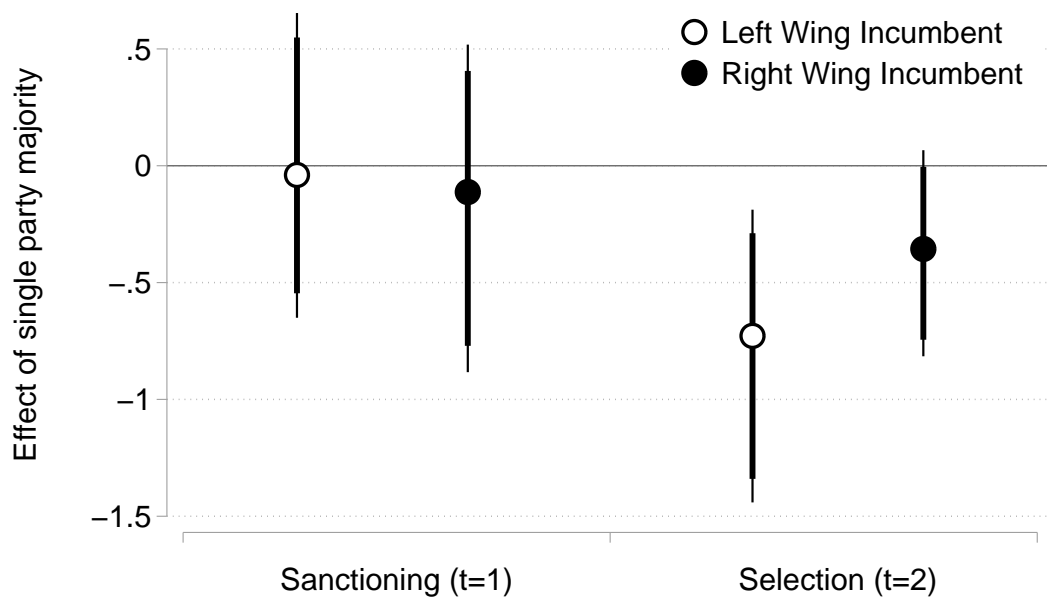


Figure S.7.7 Effects of single party majority status on tax rates for left- and right-wing parties. Spikes are 90 (wide) and 95 (thin) pct. confidence intervals. Effects are estimated using local-linear point estimation using the CCT MSE-optimal bandwidth, restricting the sample to cases where the largest party is either left- or rightwing.

S14: The Electoral Cost of Raising Local Income Tax Rates

Table S.7.14 presents four different models predicting support for the largest party using the election-year income tax rate, set by the incumbent city council, as the independent variable. All models include year fixed effects to control for trends in electoral support and income tax rates. The first two models are lagged dependent variable models with and without a set of political controls meant to index the ideological mood – using a set of dummies for mayoral ideology and the proportion of right-wing voters – as well as the political engagement of the municipal electorate – using proportion who turned out to vote. The second two models are difference-in-difference models (and thus include municipality and year fixed effects), with and without the political controls. We use both type of models to explore the robustness of our results (as recommended by Angrist and Pischke, 2008, 245).

Across all models we find the same basic result. Namely, that there is a negative and statistically significant effect of election year income tax rates on support for the largest party. The size of the effect varies slightly from specification to specification, with the largest effects being in the more restrictive difference-in-difference models.

Table S.7.14 OLS regression of largest party vote share

	(1)	(2)	(3)	(4)
Tax level, (t=1)	-0.004*	-0.005*	-0.007*	-0.008*
	(0.002)	(0.002)	(0.003)	(0.003)
Support for the largest party, t=0	0.587*	0.543*		
	(0.026)	(0.026)		
Political Controls		✓		✓
Year FE	✓	✓	✓	✓
Municipal FE			✓	✓
Observations	2180	2180	2182	2182

Municipality Clustered Standard errors in parenthesis.

Political controls: Turnout, percent right-wing voters, mayoral party ideology.

How variable is the electoral cost of raising income taxes across elections? To explore this, we estimated a version of model four from table S.7.14, where we interacted the income-tax variable with the year fixed effects. Figure S.7.8 presents marginal effects from this model across all election years included in our study (1970 is omitted since we do not have a lag of the dependent variable from before this period).

While there is some modest variation in the effect size, which drops closer to zero between '78 and '85 and is above the average from '93 to 2001, a joint significance test of the year by tax rate interactions reveal that this variation might be due to chance ($F(7, 272) = 1.31, p > 0.24$).

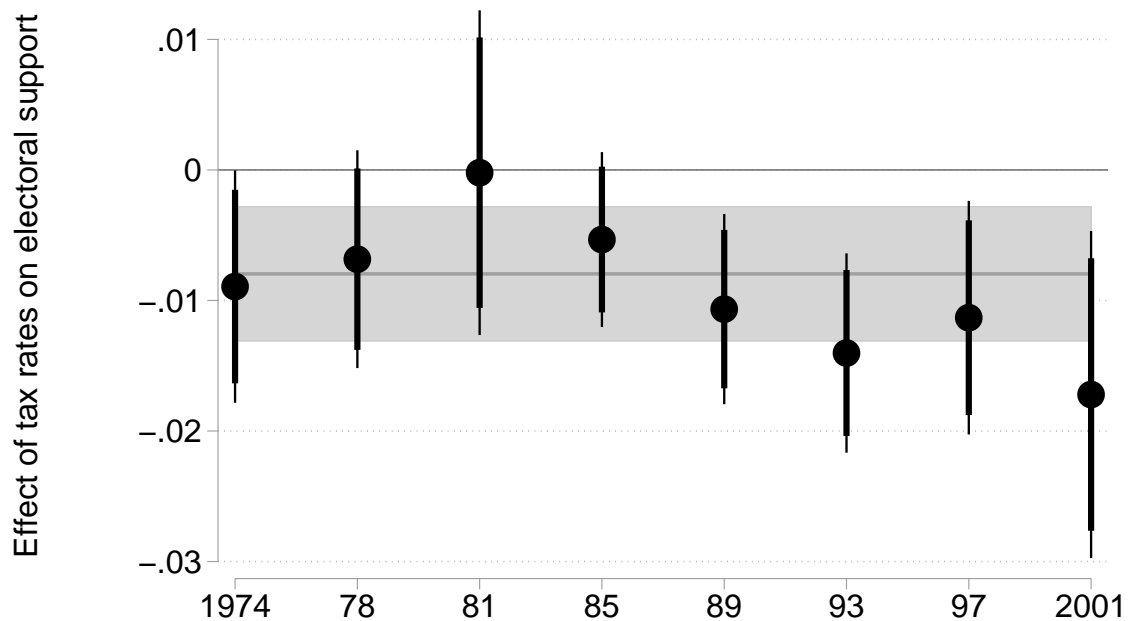


Figure S.7.8 Did voters consistently punish the largest party for raising income taxes? Marginal effects of tax rates on largest party vote share across the different elections included in our study. Spikes are 90 (wide) and 95 (thin) pct. confidence intervals. Marginal effects are derived from a difference-in-difference model including the political controls (cf. table S.7.14) as well as an interaction between the year dummies and the income tax variable. The dark gray line represents the average marginal effect across all years and the light gray area a 95 pct. confidence interval of this average effect.

Chapter 8

Incumbent Tenure Crowds Out Economic Voting

We know that economic circumstances often shape the electoral fortunes of political leaders. However, we do not know whether the importance of these economic circumstances change during political leaders' time in office. If one asserts that politicians become more responsible for the economic situation as their time in office increase, then the literature on clarity of responsibility would suggest, that there might be a positive relationship between incumbent tenure and economic voting. Conversely, if one asserts that voters continually accumulate information about politicians as their time in office increases, then models of Bayesian learning would suggest that this growing stock of information could crowd out the importance of recent economic conditions. Using three independent datasets, we empirically adjudicate between these different theoretical expectations. We find that as an executive party's tenure increases, its electoral support becomes more independent of the economic situation.

Economic circumstances often shape the electoral fortunes of political leaders (Healy and Malhotra, 2013; Lewis-Beck and Stegmaier, 2013). A recession, a spike in unemployment, or an increase in prices, will, all other things being equal, lead voters to unseat incumbent politicians and elect political opponents. However, other things are often not equal. Studies on the economic antecedents of electoral behaviour have identified extensive variation, in the relationship between the economic situation and electoral support for incumbents (Duch and Stevenson, 2008; Lewis-Beck, 1990; Paldam, 1991; Van der Brug, Van der Eijk and Franklin, 2007). So why is economic voting more prevalent in some elections than it is in others? Previous studies of why the economic vote varies have typically explained this in terms of variations in the political and economic institutional context (e.g. Duch and Stevenson, 2008; Powell and Whitten, 1993). In particular, these studies have found that institutions that regulate the extent to which incumbents have unified control over economic policy condition the extent to which incumbents are held electorally accountable for the economic situation. For instance, works in this ‘clarity of responsibility’ literature have found that economic openness (Hellwig and Samuels, 2007; Hellwig, 2001) and government composition (Anderson, 2006; Hobolt, Tilley and Banducci, 2013) moderate the economic vote. Yet, despite this comprehensive literature on the sources of variation in economic voting, a surprisingly understudied question is whether the duration of a government’s time in office conditions the economic vote.

Only a small number of studies have dealt with the relationship between time in office and economic voting. These studies have generally conceptualised tenure as a component of clarity of responsibility (Nadeau, Niemi and Yoshinaka, 2002), asserting that incumbents’ influence on economic outcomes increases with their time in office, and because of this one should expect economic voting to be *more* prevalent in elections which feature more experienced incumbents and *less* prevalent in elections which feature less experienced incumbents. Empirically, these studies have primarily, though not exclusively, been interested in the short term relationship between economic voting and tenure, studying how the economic vote develops during an incumbent’s first term (Carey and Lebo, 2006; Lebo and Box-Steffensmeier, 2008; Singer and Carlin, 2013). Generally, they find that retrospective economic voting tends to increase during the incumbent’s first two years in office. In this article, we add to this small, growing literature by employing an alternative theoretical approach to the relationship between tenure and the economic vote, Bayesian learning, and by examining the long term relationship between economic voting and time in office using a number of different data sources.

Models of Bayesian learning tells us that beliefs which are based on more information are generally less likely to be moved by a single new piece of evidence than beliefs which are based on less information (Breen, 1999; Gerber and Green, 1999). In the context of economic voting, one can conceptualise this “information” as all relevant cues about the quality of the incumbent obtained before one takes recent economic conditions (i.e., the new piece of evidence) into account. Information naturally accumulates with time in office. That is, voters will always have more information about their incumbent at $t = x + 1$ than they have at $t = x$, because all of the information accumulated by $t = x$ is also available at $t = x + 1$. Accordingly, as an incumbent’s time in office increases, voters stock of relevant information increases, strengthening voters beliefs about the incumbent, leaving the beliefs less malleable, and thus potentially attenuating the effect recent economic conditions have on these beliefs.

If one considers Bayesian learning in conjunction with the clarity of responsibility hypothesis that has been advanced in previous research, it becomes clear that there could be countervailing forces at work when it comes to the relationship between economic voting and time in office. As such, the clarity of responsibility hypothesis suggests that experienced incumbents have more influence over the economy than inexperienced incumbents, driving economic voting up as time in office increases, whereas Bayesian learning suggests that voters’ beliefs about inexperienced incumbents are more malleable than their beliefs about experienced incumbents, driving economic voting down as time in office increases.

In order to determine which of these countervailing forces dominate, this article sets out to conduct a thorough, empirical re-examination of the long term relationship between economic voting and time in office. We begin this re-examination by studying country-level election returns from 409 elections in 41 different countries. Measuring economic voting as the correlation between economic conditions (i.e. economic growth) and support for the executive party, we find that as incumbent tenure increases, economic voting decreases. Next, we conduct a conceptual replication, where we once again identify a negative relationship between time in office and economic voting. The conceptual replication utilizes a pooled cross-section of 60 representative national surveys from 10 Western European countries, measuring the economic vote by correlating voters’ retrospective perceptions of the national economy and their intention to vote for the executive party. Our third and final study uses subnational data to zero in on the proposed theoretical mechanism driving the negative relationship between time in office and economic voting: that is, Bayesian

learning. In particular, we look at a dataset of some local elections in Denmark that were held in the wake of a large municipal redistricting reform. We take advantage of the fact that, as a consequence of the reform, a number of municipalities were amalgamated, which created within-municipality differences in the amount of experience the electorate had with the same incumbent mayoral party. We find that voters who had less experience with their incumbent mayor were more likely to hold the mayor electorally accountable for recent changes in local levels of unemployment. In effect, we find that as voters become more acquainted with an incumbent, they become less inclined to hold this incumbent electorally accountable for recent economic conditions

These results are surprising for a couple of reasons. First, because some previous studies have found that economic voting increases with tenure. A divergence which might be explained by the fact that previous researchers have primarily examined how economic voting changes in the first year or two of the incumbent's time in office, whereas our study examines the more long term effects of tenure. Second, because the clarity of responsibility hypothesis has such a dominant position in the comparative economic voting literature, one might have suspected that it also had extensive explanatory power when it came to explaining the effects of incumbent tenure. This does not seem to be the case. It is important to note that this of course does *not* mean that clarity of responsibility is irrelevant for explaining why the economic vote varies from one election to another. The clarity of responsibility hypotheses have been shown, time and again, to be quite apt for predicting variation in economic voting. Even so, these results highlight the importance of considering other factors than just clarity of responsibility when trying to understand how and why the economic vote varies.

This article extends the literature on comparative economic voting — a literature which has, broadly speaking, focused on either political institutions (e.g., Duch and Stevenson, 2008; Hobolt, Tilley and Banducci, 2013; Powell and Whitten, 1993; Whitten and Palmer, 1999) or voter characteristics (e.g., Kayser and Wlezien, 2011; Malhotra and Kuo, 2008; Vries and Giger, 2014). In particular, the article helps analysts and researchers interested in understanding why economic voting is more prevalent in some elections than in others, by giving them an additional factor to take into account when making predictions about how important the economy will be in a particular election: the tenure of the executive up for reelection. Further, by highlighting Bayesian learning as a key to understanding why incumbent tenure crowds out economic voting, this article also demonstrates that in spite of voter

myopia (Healy and Lenz, 2014; Healy and Malhotra, 2009), incumbents' history have an important role to play if one wants to understand the temporal dynamics of electoral accountability (see Krause and Melusky 2014 for a similar point).

Time in Office and the Economic Vote

Research on how the economy shapes the electoral support for incumbents has generally been sensitive to the role that the timing of economic outcomes might play for both voters and politicians. For instance, several studies have investigated the extent to which voters are myopic, caring exclusively about present (and recent) economic conditions (Achen, 2012; Healy and Lenz, 2014; Healy and Malhotra, 2009; Hellwig and Marinova, 2014), and the degree to which incumbent politicians can use this myopia strategically by creating political business cycles and when timing elections (Kayser, 2005; Nordhaus, 1975; Samuels and Hellwig, 2010; Smith, 2003). Other studies have tried to discern whether voters focus on past or (their expectations of) the future economy (Lanoue, 1994; Soroka, Stecula and Wlezien, 2015). Yet the existing research on economic voting has rarely examined the potential role politicians' time in office might have in moderating the economic vote. Instead, those interested in tenure have examined whether and why there is a general cost of ruling (Nannestad and Paldam, 1994; Paldam and Skott, 1995; Stevenson, 2002), and other types of generalisable temporal trends in the incumbent's popularity (Mueller, 1970).

There are at present only a few studies which examine time in office in conjunction with the economic vote. One of these studies is by Nadeau, Niemi and Yoshinaka (2002). They include time in office in a larger index of 'dynamic clarity of responsibility' (i.e., ideological cohesion of the government, the number of parties in parliament), and then look at whether this index correlates with the economic vote in eight different European countries. They find a positive relationship between economic voting and their index, but they do not examine time in office separately from the other factors. Studies by Carey and Lebo (2006) and Lebo and Box-Steffensmeier (2008) examine how the nature of economic voting changes across the election cycle. Focusing on the US and the UK respectively, they tend to find more prospective economic voting at the beginning of an election cycle and more retrospective economic voting at the end of an election cycle, however, their analyses are primarily graphical, and they conduct no formal statistical tests of the differences they identify.

The most thorough examination of the relationship between time in office and the economic vote can be found in a recent study by Singer and Carlin (2013). Here,

they link time in office with different types of economic voting in a wide cross-section of Latin American countries. They find that “voters’ reliance on prospective expectations indeed diminishes over the election cycle as the honeymoon ends and they retrospectively evaluate the incumbent’s mounting record” (Singer and Carlin, 2013, 731). Even though this study is well executed and convincing, the inferences one can make about the long term relationship between tenure and economic voting based on this study are limited by two factors. First, the study measures economic voting by looking at economic perceptions, not objective economic conditions. Second, and more importantly, their focus is on the short term relationship between time in office and the economic vote. This is in part because the study focuses on a region of the world which is relatively politically volatile: most of the incumbents examined have only been in office a short time. Roughly 90 percent of the incumbents they examine have held office for less than five years, and the median time in office is 2.5 years.¹ The authors are aware of this limitation, and their theoretical predictions, as well as their key findings, tend to be concerned with the first few years of the incumbent’s time in office (Singer and Carlin, 2013, figure 1, 738).

Taken together, this small set of studies have made important headway in exploring the relationship between time in office and the economic vote, but they leave some important empirical questions unanswered. First, what is the *long term* relationship between tenure and the economic vote? In many countries the same incumbent has been in power for many years – sometimes more than a decade. While existing studies tell us something about how economic voting evolves through the first election cycle, we know little about what happens beyond that. Is there, for instance, a difference between an incumbent who has been in office for four years and one who has been in office for 10? At present we do not know. Second, is there a relationship between the extent to which objective economic conditions affect support for the incumbent and time in office? Existing studies have exclusively focused on how the effects of prospective versus retrospective economic perceptions change as time in office increases; however, we do not know whether the effect of objective economic conditions changes with time in office.

Theoretical Expectations

On a theoretical level, existing studies have explained why tenure moderates the economic vote in terms of the clarity of responsibility hypothesis. First developed

¹See section S2 of the supplementary materials for the distribution of time in office in the Singer and Carlin (2013) study.

by Powell and Whitten (1993), the clarity of responsibility hypothesis suggests that the extent of economic voting depends on the extent to which governments are, or seem to be, responsible for economic outcomes (see also Duch and Stevenson, 2008; Fisher and Hobolt, 2010; Hellwig, 2001; Lobo and Lewis-Beck, 2012). Based on this hypothesis, previous research has concluded that it seems reasonable to expect, that as an incumbent's time in office increases, so does the economic vote. The underlying logic is that incumbents with more tenure will have had more time to enact policies that affect economic conditions (Nadeau, Niemi and Yoshinaka, 2002). Voters recognise this, and punish and reward incumbents more for the state of the economy as time in office increases.

As mentioned in the introduction, this intuition becomes more complicated if one takes the concept of Bayesian learning account. Bayesian learning asserts that when people make inferences, these are based upon a set of *prior* beliefs, which are then updated when people encounter a new piece of evidence (Gerber and Green, 1999; Granato et al., 2015). In the context of economic voting, this would mean that when voters evaluate the incumbent, they base their evaluation on some prior beliefs about the incumbent's quality, which they then update when they observe the economic situation (the new piece of evidence). A key prediction from models of Bayesian learning is that the extent to which people rely on new evidence when forming their beliefs depends on how strong their prior beliefs are. If their prior beliefs are weak (i.e., rely on little relevant information) then new evidence will have more of an impact than if their prior beliefs are strong (i.e., rely on a lot of relevant information).

What implications does this have for the relationship between economic voting and tenure? Imagine that a new incumbent has just come into office, and after four years they then run for re-election. Because the incumbent is relatively new, voters' prior beliefs about the incumbent's competence are not based on a lot of solid information. The economic situation around election time is good, and since voters have no information to the contrary, they infer that the incumbent is probably a competent economic manager. The incumbent is re-elected for a second term, and runs for re-election once again. Now the economic situation is bad. If voters were to put the same weight on the economic situation as they did before the previous election, they would conclude that the incumbent is probably an incompetent economic manager. However, going into second term they believe, because of the incumbent's economic record in the first term, that the incumbent is probably competent. Their prior beliefs have strengthened

This ends up moderating the voters judgement, as they conclude that given the incumbent's history of producing both good and bad economic outcomes, the incumbent is probably a mediocre economic manager. Put differently, because voters take prior information about the incumbent into account, the economic situation has less of an impact on voters beliefs about the incumbent when the incumbent runs for re-election a second time (in section S1 of the supplementary materials, we formalise this argument, showing that this belief formation process is rational). Because voters' stock of information about the incumbent will always increase with time in office, voters' beliefs will continually strengthen. As such, we can say that if voters are Bayesian learners, time in office will tend to crowd out the electoral importance of recent economic conditions.

Bayesian learning is not incompatible with the clarity of responsibility hypothesis. Voters might think the incumbent becomes more responsible for the economic situation as their time in office increases, while at the same time interpreting the present economic situation in light of their prior experiences with the incumbent. In section S1 of the supplementary materials we present a formal model in which both Bayesian learning and clarity of responsibility are at work simultaneously. The key take-away from this model is that it is not possible to arrive at a uniform theoretical prediction about whether economic voting will increase or decrease as time in office increases. Instead, the model shows that what force comes to dominate will depend on exactly how much voters believe clarity of responsibility increases with time in office, and how much voters believe they can generally infer about the incumbent's competence from economic conditions.

This theoretical ambiguity, combined with the empirical limitations of the existing literature, motivate the empirical examination of the relationship between time in office and economic voting which takes up the remainder of the article.

Country-level Evidence

We begin our exploration of the relationship between tenure and the economic vote by examining a country-level dataset of national elections. This type of data has been used to study variation in the economic vote in numerous other studies (cf. Hellwig and Samuels, 2007; Kayser and Peress, 2012; Powell and Whitten, 1993; Whitten and Palmer, 1999). The underlying assumption in these studies is that one can measure the level of economic voting by looking at the correlation between economic indicators and electoral support for incumbents, and, in turn, use variations in this correlation

to infer whether specific factors, such as tenure, affect the degree to which voters hold politicians accountable for economic outcomes.

The chief advantage of this approach is that it sidesteps problems of endogeneity related to using voters' *perception* of the economy by using objective economic indicators instead (Kramer 1983, Van der Brug, Van der Eijk and Franklin 2007, 26). The chief disadvantage is that the economic indicators used are country-level aggregates. These aggregates are noisy estimates of the economy as experienced by the individual voter (Duch and Stevenson, 2008, 26), and they are restricted to $n = 1$ per election, limiting the statistical power of the analysis. To overcome these problems, we use a relatively large sample of elections and, later in the article, we replicate our findings using an individual-level data set which includes a subjective measure of economic conditions.

Data and Model

We use a dataset of 409 elections across 41 different countries (see section S2 of the supplementary materials for a list of the countries and elections). To get such a wide cross section of elections, we use and amend datasets already developed by Kayser and Peress (2012) and Hellwig and Samuels (2007). The key dependent variable is the percentage point change in electoral support for the *executive party* at legislative and executive elections (Δy).² The executive party is the party which had primary control of the executive branch at the time of the election (i.e., the party of the prime minister or the president). Using the executive party rather than the parties in government is common in the literature (see, for instance, Duch and Stevenson 2008). Further, several studies have shown that the executive party is much more prone to electoral judgement than other governing parties (Debus, Stegmaier and Tosun, 2014; Fisher and Hobolt, 2010; Van der Brug, Van der Eijk and Franklin, 2007).

The key independent variables are economic growth (gr) and tenure (ten). Economic growth is a proxy for the economic conditions in the country and is measured as election-year GDP per capita growth (as a percentage, pct.). This indicator is used because it is available for a large cross section of elections and because it has been widely used in the previous literature. For elections occurring in the first six months of the year, we use economic growth in the year prior to the election year, for elections

²In presidential systems, where the president is directly elected by the voters, we use both support for the president at presidential elections, as well as support for the president's party in the legislature. Since our data is primarily from parliamentary systems, we end up with 57 executive elections and 352 legislative elections. The legislative election results used are from the lower house if the legislature is bicameral.

occurring in the final six months of the year we use economic growth in the election year. Data on economic growth was taken from the World Bank's database. Time in office is measured as the number of years since the current executive party came into power. We focus on the tenure of parties, since the main dependent variable is support for the executive party. Data on tenure is taken from the database of political institutions (Beck et al., 2001), and has been extended by the author to create better coverage for the electoral variables. The average level of tenure for the incumbent parties is six years, and the median is five years. See section S3 of the supplementary materials for descriptive statistics on all of the variables.

Turning to modeling, we set changes in electoral support as a linear function of tenure, economic growth and an interaction between the two. We also include a dummy variable indicating whether the election is executive or legislative (*exec*) to take into account that economic voting works differently in executive and legislative elections (Hellwig and Samuels, 2008; Samuels, 2004). As such, the baseline model we estimate can be described as

$$\Delta y_{it} = \beta_0 + \beta_1 gr_{it} + \beta_2 ten_{it} + \gamma gr_{it} \times ten_{it} + \beta_3 exec_{it} + \epsilon_{it}. \quad (8.1)$$

The coefficient of interest is γ , which signifies the change in the effect of economic growth as tenure increases. If the coefficient is negative, it means economic voting decreases with time in office. If the coefficient is positive, it means that economic voting increases with time in office. In the analysis below, we add various statistical controls to this baseline model, in order to try and sieve out any potential confounding.

Results

Table 1 presents key estimates from the model described in equation 8.1 in column one, using a maximum likelihood estimator to obtain country-clustered standard errors. The baseline growth and tenure effects should be interpreted as the effect of the variable when the other variable is held at zero. The baseline effect of economic growth is thus estimated to be 0.68, and can be understood as the (theoretical) effect of economic growth on change in electoral support if an incumbent runs for re-election without any tenure.

The variable of interest is the interaction between economic growth and tenure. The interaction is statistically significant and negative, suggesting that the positive effect of economic growth at the beginning of an executive party's tenure diminishes over time. Specifically, the estimate suggests that each year, the effect of economic

growth on electoral support drops by 0.06 from the starting point of 0.68. Accordingly, this model suggests that after 12 years in office, the effect of economic growth is essentially zero.

Table 8.1 Linear regression of changes in executive party vote share

	(1)	(2)	(3)	(4)
Economic growth	0.68*	0.78*	0.61*	0.90*
	(0.25)	(0.28)	(0.29)	(0.30)
Tenure	-0.05	-0.03	-0.28 ⁺	-0.25
	(0.15)	(0.13)	(0.15)	(0.17)
Economic growth × Tenure	-0.06*	-0.07*	-0.05	-0.06*
	(0.03)	(0.03)	(0.03)	(0.03)
Executive election	-2.80 ⁺	-1.81	-0.90	-4.12*
	(1.52)	(1.51)	(1.97)	(1.17)
Year FE		✓	✓	
Country FE			✓	✓
Leader FE				✓
Observations	409	409	409	409

Standard errors in parentheses

Standard errors clustered by country.

⁺ $p < 0.10$, * $p < 0.05$

How sensitive is this finding to different model specifications? To investigate this, the baseline model is extended in three ways. In column two, we show estimates from a model including year fixed effects. These take any global trends in growth, tenure and incumbent support into account. This leaves the interaction practically unchanged.

The second extension is the addition of country fixed effects. These control for potentially confounding differences in tenure and economic growth across different countries. In column three we show estimates from a model including country fixed effects, and while these do not change the interaction effect markedly, it does drop 0.01 from the baseline specification, leaving the estimate statistically insignificant.

The third extension is the inclusion of leader fixed effects: that is, a dummy for each of the 158 different incumbents in the dataset.³ Including the leader fixed effects means that any factors which are constant within the same incumbent are omitted when estimating the interaction. As such, the model estimates the interaction by comparing the degree to which the same executive party is punished (or rewarded) for the economic situation across elections, rather than comparing how harshly different executive parties with different levels of tenure are punished. The leader

³The leader fixed effects count an executive party which returns to power after being defeated as a new incumbent. For instance, the United Kingdom has five different incumbents in the dataset across 11 elections, in spite of the fact that only two different parties were in power across these elections.

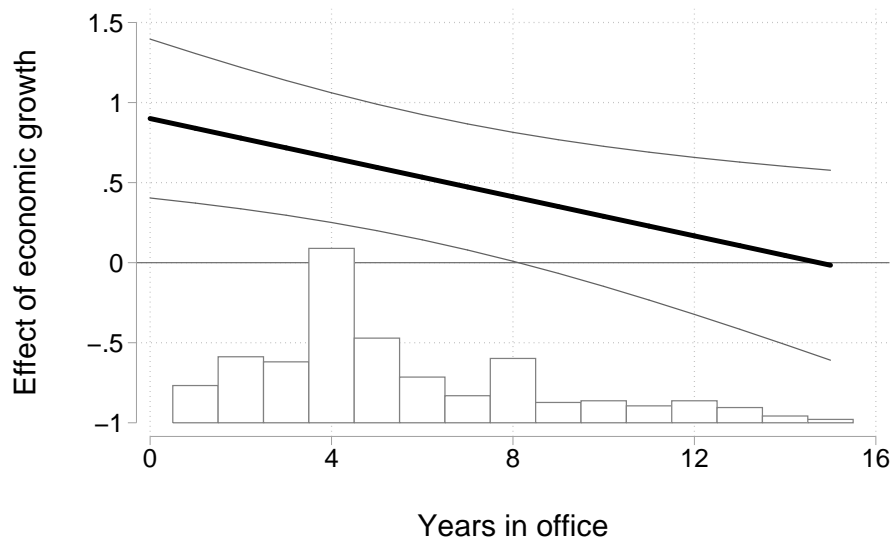


Figure 8.1 Marginal effects of economic growth on the change in electoral support for the executive party across levels of tenure with 90 pct. confidence intervals. We only plot tenure from the 5th to the 95th percentile. Derived from the model presented in column four of table 8.1. The bar plot shows the density of the variable years in office.

fixed effects make the year fixed effects less relevant, as we are now comparing levels of economic voting across a relatively short span of time (i.e., from the beginning to the end of an incumbent's tenure). Further, if they are included along with the leader fixed effects the degrees of freedom drop dramatically, and they are therefore omitted from the model with the leader fixed effects. The leader fixed effects are included in the model estimated in the fourth column of table 8.1. In this specification, the interaction estimate is virtually unchanged and statistically significant at the five percent level. Figure 8.1 plots the marginal effects of economic growth across time in office using this specification.

In the supplementary materials, four additional robustness checks are laid out. First, we look at whether the results are sensitive to using the average growth rate across the previous two years rather than simply the election-year growth rate. This does not substantially affect the results (see section S4 of the supplementary materials). Second, we look at whether adding additional controls for parliamentary and government composition affects the results. This means omitting a large number of elections for which this information is not available, increasing the standard errors attached to the estimates. Even so, the interaction estimates are not affected by adding

the controls (see section S5 in the supplementary materials for details). Third, we look at whether a single country is driving the results. We find that the interaction estimates in columns one and two are not sensitive to excluding a single country. For the models in columns three and four, the exclusion of Luxembourg draws the interaction closer to zero, however, the interaction remains negative even when excluding this country (see section S6 of the supplementary materials for details). Fourth, we examine the interaction between economic growth and tenure in light of the different diagnostics suggested by Hainmueller, Mummolo and Xu (2016). This is done in section S7 of the supplementary materials. Overall, we find monotonicity in the average marginal effects and approximate linearity. However, we also find that the interaction variable is kurtotic, which hurts the reliability of the interaction estimate.

In conclusion, our analyses of the country-level data suggests that economic voting decreases with time in office. The long term relationship between economic voting and tenure is negative. Even so, the estimated interaction effect was not consistently statistically significant. This can, in part, be explained in terms of the low statistical power of country-level analyses. As mentioned above, the chief disadvantage of using country-level data is that it is quite noisy. To address this, we conduct a conceptual replication with individual-level data in the next section.

Before moving on to the replication, however, a few alternative explanations for the findings deserve to be discussed. For one, the negative correlation between tenure and economic voting might be due to strategic election-timing (Kayser, 2005; Samuels and Hellwig, 2010). That is, what we see above might simply be a reflection of the fact that certain types of leaders call early elections, and are therefore more likely to have less tenure when they run. In the supplementary material, we examine this alternative explanation by trying to control away election-timing in two different ways: (1) by including a control for how often incumbents call elections and (2) by restricting the sample of elections to countries with fixed terms, where strategic election timing is not possible. In both cases, the most demanding specification, which includes leader fixed effects, produces an interaction estimate which is negative and statistically significant (see section S8 of the supplementary materials for details). Another possible alternative explanation for our findings might be that the negative interaction can be explained by the fact that voters start out by holding the executive party electorally accountable, but then, as time goes by, voters start to hold government coalition partners accountable as well. To test whether this is the case, we estimate the models from table 8.1 separately for coalition governments and single

party governments in section S9 of the supplementary materials. We identify no systematic differences across the two groups, suggesting that the negative interaction term cannot be explained by voters holding coalition partners more accountable as time in office increases. Finally, we look at whether our results can be ascribed to the fact that we study incumbent parties (e.g. Labour), rather than executive officers (e.g., Tony Blair). To do this, we add a control to the model for whether the incumbent party and the executive officer have different levels of tenure. The results, reported in section S10 of the supplementary materials, show that this does not shift the interaction estimates substantially, however, the level of statistical significance does drop from 0.05 to 0.1.

Individual-level Evidence

Having established a relationship between economic voting and the tenure of the executive party at the country-level, we now explore the same relationship at the individual-level. In essence, we try to replicate our results by investigating whether voters rely less (more) on their perceptions of how the national economy has developed when deciding whether to vote for a more (less) experienced incumbent.

To do this, we closely follow a recent study by Nadeau, Lewis-Beck and Bélanger (2013), who investigated the relationship between national economic perceptions and the vote for executive parties in 10 Western European countries over the past 20 years. This gives us a well-established model of the economic vote, and allows us to simply extend this model to include an interaction between tenure and economic perceptions.

It is important to explain, what we hope to gain from this replication. Because results in the comparative economic voting literature are known to be quite unstable (Paldam, 1991), it makes sense to replicate our findings using a new dataset. As such, if we get similar results using a new dataset, it makes it more likely that what we found above is a generalisable pattern. A replication of our country-level results using individual-level economic perceptions is particularly relevant in light of the studies described above (i.e., Carey and Lebo, 2006; Singer and Carlin, 2013), which have found a positive relationship between the effect of economic perceptions and tenure (at least in the short term). Finally, by using a set of surveys which run outside national election cycles, it is possible to sidestep any additional concerns one might have about the extent to which the identified relationship between tenure and economic voting is driven by election-timing.

Data and Model

We use the European Election Studies (ESS). The EES is a survey of all EU countries which has been conducted every fifth year since 1979. They are fielded in the year of European Parliamentary elections, and their timing is therefore somewhat independent of national elections. We use the six Europe-wide studies which have been conducted since 1989 (i.e., '89, '94, '99, '04, '09, and '14), as these are the only surveys which include questions about national economic perceptions as well as vote intention in national elections. Moreover, we focus on the ten countries which have participated in all six survey-rounds: Denmark, France, Germany, Greece, Ireland, Italy, the Netherlands, Portugal, Spain and the United Kingdom (see section S2 of the supplementary materials for details about the samples used). This gives us 60 cross-sectional national surveys, which can be pooled to test whether the effect of economic perceptions on voter intentions depend on the tenure of the executive party.

Turning to indicators, the key dependent variable is whether respondents report that they would vote for the executive party if a national legislative election was held tomorrow (*reelect*).

The key independent variables are national economic perceptions and tenure. National economic perceptions (*NEP*) are measured using a question that asked respondents whether the economic situation in their own country had gotten better or worse in the past 12 months. Responses were recorded on a five-point scale (except for the 1994 election study, which used a four-point scale). Tenure (*ten*) is measured as the number of years the executive party had been in power at the time of the survey. Once again, this variable is taken from Beck et al. (2001) and extended to provide complete coverage for the 60 surveys. The mean time in office of the incumbent parties is five years and the median is four years.

We use the same control variables as Nadeau, Lewis-Beck and Bélanger (2013) use for their standard economic voting models: respondents' ideology, self-perceived class, church attendance and a dummy indicating whether the respondent voted for the executive party at the last election.⁴ All variables were rescaled to go from zero to one, and recoded so that higher values were likely to mean a higher probability of

⁴We exclude a control used by Nadeau, Lewis-Beck and Bélanger (2013) measuring the time since the last election, since this variable is very closely related to tenure.

voting for the executive party.⁵ See section S3 of the supplementary materials for the exact question wording and descriptive statistics.

We model the probability that voters will report an intention to vote for the executive party as a logistic function of national economic perceptions, tenure, an interaction between the two and the individual level controls. As such, the model we estimate can be described as

$$Pr(reelect) = \text{logit}(\alpha_0 + \alpha_1 NEP_{ijt} + \alpha_2 ten_{it} + \gamma ten_{it} \times NEP_{ijt} + \mathbf{X}_{ijt}\beta + \epsilon_{ijt}), \quad (8.2)$$

where i indicates country, t year and j the respondent. \mathbf{X} is a row vector of the control variables *ideology*, *class*, *religion* and *reelect_{lag}* and β is a column vector of coefficients attached to these controls. The coefficient of interest is once again γ , which signifies the change in the effect of national economic perceptions as tenure increases. Following our country-level analysis, we expect γ to be negative.

Results

In the first column of table 8.2, we estimate the parameters of the model presented in equation 8.2 using a multi-level logistic regression. We cluster the standard errors at the country-level and estimate random effects at the survey-level.

Ideology, class, religiosity and lagged executive party vote all have the expected signs, and, apart from religiosity, are statistically significant. The baseline economy and tenure effects should (once again) be interpreted as the effect of the variable when the other variable is held at zero. The baseline effect of national economic perceptions is estimated to be 1.85, and can thus be understood as the (theoretical) effect of going from one end of the national economic perceptions scale to the other on the logit probability of voting for an executive party without any tenure.

The key estimate of interest is the one attached to the interaction between national economic perceptions and tenure, which signifies how the effect of national economic percepts change as tenure increases. The interaction-coefficient is statistically significant and negative, suggesting that the positive effect of the respondents' perception of the national economy at the beginning of an executive party's tenure diminishes as their time in office increases – an interaction effect which is qualitatively similar to the one found in the country-level data.

⁵In particular, religion, class, and ideology were coded differently across the different surveys to take differences in the ideological position of the executive into account.

Table 8.2 Multi-level logit model of voting for executive party

	(1)	(2)	(3)
National Economic Perceptions	1.85*	1.89*	1.88*
	(0.22)	(0.22)	(0.22)
Tenure	0.05 ⁺	-0.02	
	(0.03)	(0.04)	
National Economic Perceptions × Tenure	-0.05*	-0.06*	-0.06*
	(0.02)	(0.02)	(0.02)
Lagged executive party vote	4.36*	4.37*	4.37*
	(0.16)	(0.16)	(0.16)
Ideology	2.31*	2.30*	2.32*
	(0.24)	(0.24)	(0.24)
Religiosity	0.13	0.14	0.14
	(0.16)	(0.16)	(0.16)
Class	0.30*	0.30 ⁺	0.30 ⁺
	(0.15)	(0.16)	(0.16)
Survey RE	✓	✓	✓
Leader FE		✓	✓
Survey FE			✓
Observations	39,556	39,556	39,556

Standard errors in parentheses

Standard errors clustered by country.

Tenure omitted in model (3) due to collinearity with Survey FE.

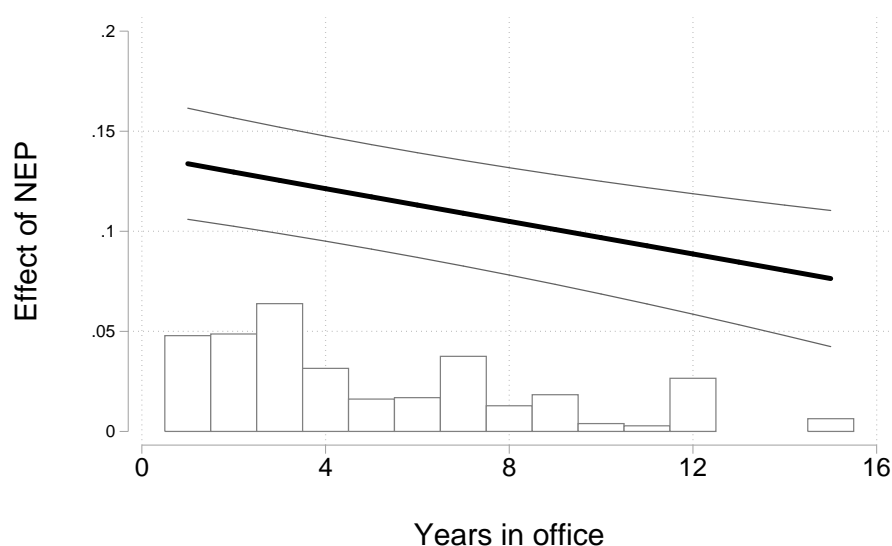
⁺ $p < 0.10$, * $p < 0.05$ 

Figure 8.2 Average marginal effects of national economic perceptions on the probability of voting for the executive party across levels of tenure with 90 pct. confidence intervals. We only plot tenure from the 5th to the 95th percentile. Derived from the model presented in column 3 of table 8.2. The bar plot shows the density of the years in office variable.

We also investigate whether these individual-level findings are sensitive to different model specifications. In column 2, we include leader fixed effects (cf. the country-level data). Estimating this more demanding model does not substantially change the results. The interaction remains negative and statistically significant. In column 3, we introduce survey fixed effects; a dummy for each of the sixty surveys. The interaction between national economic perceptions and tenure remains negative and statistically significant in this model as well.

In order to investigate the consequences this negative logistic interaction has for the relationship between voters' economic perceptions and their propensity to vote for the executive party, we derive average marginal effects of these perceptions across different levels of tenure based on the model with survey fixed effects. These average marginal effects are plotted in figure 8.2. This figure reveals that the average marginal effect of national economic perceptions is reduced as tenure increases. For an executive party with one year of tenure, the effect of a voter going from one end of the economic perception scale to the other is an increase in the probability of voting for the executive party of about 14 percentage points. For an executive party with 15 years of tenure the same change leads to an increase of eight percentage points. A comparison of the average marginal effect at one years of tenure and fifteen year of tenure reveals that this decline is statistically significant ($p < 0.01$).

In the supplementary materials, we conduct a number of additional robustness checks for this finding. We show that the main results are robust to a two-step estimation procedure, cf. section S11. We show that the results are not sensitive to outliers, cf. section S6. We also examine the robustness of the interaction in light of Hainmueller, Mummolo and Xu (2016), cf. section S7.

In the model above, we use a standard retrospective question, which asks voters how their country's economy has developed in the past year. However, some studies of American elections have suggested that when an executive party has been in office for a while, retrospective concerns give way to prospective concerns (Nadeau and Lewis-Beck, 2001). That is, voters beliefs about how the economy is going to develop becomes more important than their beliefs about how the economy has developed in the past (although see Carey and Lebo, 2006; Singer and Carlin, 2013, for the opposite argument). Based on this, one might suspect that the reason we see a drop in the effect of retrospective economic perceptions is that the *type* of perceptions which matter at the beginning of the term are different from those that matter at the end of term. To test whether this is the case, we examine the relationship between prospective national economic perceptions and time in office in section S12 of the

supplementary materials. We find that the effect of prospective economic perceptions are not moderated by time in office. As such, there are no signs that some other type of economic percepts become more important as the effect of retrospective national economic percepts subside.

Overall, the results seem to line up nicely with what we found in the country-level data, however, there is one important inconsistency. While both datasets show the importance of the economic vote decreasing with time in office, the decline seems to be less dramatic in the individual-level data. In the country-level data, the estimated effect of the economy is essentially zero after 15 years (cf. figure 8.1). In the individual-level data, there is still a substantial amount of economic voting left after 15 years (cf. figure 8.2). One potential explanation for this inconsistency is that the individual-level data overestimates the amount of economic voting across all levels of tenure.

There are some studies that suggests that we do, in general, overestimate economic voting, when using voters' perceptions of the economy rather than objective economic conditions (Evans and Pickup, 2010; Evans and Andersen, 2006; although see Lewis-Beck, Nadeau and Elias, 2008). In particular, these studies argue that partisan voters will adjust their perceptions of the economy to make their preferred party look good, which leads to inflated estimates of the economic vote (Gerber and Huber, 2010; Tilley and Hobolt, 2011). Put more succinctly, economic perceptions are endogenous and this might explain the discrepancy between the individual-level and country-level results.

In section S13 of the supplementary materials, we try to correct for this type of endogeneity in two ways. First, we re-analyze the survey data excluding potential pro-executive partisans. Second, we use objective economic conditions as instruments of national economic perceptions. In both cases, we find that correcting for endogeneity tends to align the results from the individual-level with the results from the country-level data, suggesting that the immediate divergence between the country-level and the individual-level results can be explained by the methodological idiosyncrasies of how the economic vote is measured in the two different datasets.

Taken together, the individual-level findings reaffirm the country-level findings: as an incumbent party's time in office increases, the economy becomes a less important determinant of their electoral fortune.

Subnational Evidence

So far, our empirical analyses suggest that for a large cross section of countries and elections, there is a negative long term relationship between economic voting and time in office. Why is this the case? In our discussion of theoretical expectations, we advanced one reason why incumbent tenure might be driving down economic voting: voters' stock of information about the incumbent naturally increases with time in office. Following models of Bayesian learning, we surmised that this accumulation of information would strengthen voters' beliefs about the incumbent, making them less malleable to the economic situation. That is, the economic situation will play less of a role in shaping voters beliefs about an incumbent as time in office increases, because the economic situation becomes an increasingly small part of the total stock of relevant information voters have about the incumbent. In this third and final study, we try to examine this potential explanation of the country-level and individual-level findings in more detail.

To do this, we focus on a set of municipal elections following a 2005 jurisdictional reform of local government in Denmark, in which a large number of municipalities merged (for details on the reform, see Bhatti and Hansen, 2011; Blom-Hansen, Houlberg and Serritzlew, 2014; Lassen and Serritzlew, 2011). This reform allows us to isolate variation in voters stock of information about the incumbent – the key factor we believe is driving down economic voting as time in office increases – while holding attributes of the political system, the election, and the incumbent constant. In particular, this reform allows us to separate the amount of experience voters have with an incumbent from the amount of experience an incumbent has with being in office.

To see how we can use the reform in this way, consider the following stylized example. Municipality 1 and municipality 2 merge as a result of the jurisdictional reform. Before the merger, party A was the mayoral party in municipality 1, whereas party B was the mayoral party in municipality 2. In 2005, these municipalities merge and have to elect one, common mayoral party. They elect party A. In the following election (i.e., 2009), the voters in the newly merged municipality have to decide whether to re-elect the incumbent party A. The voters who originally lived in municipality 1 have accumulated information about this mayoral party both before and after the merger. The voters who originally lived in municipality 2 have only accumulated information about this mayoral party after the merger. Figure 8.3 visualizes this example.

What are our expectations if voters' stock of information drives down the economic vote? In terms of the stylized example, we should expect economic voting to be less prevalent among voters who originally lived in municipality 1, and more prevalent among those who originally lived in municipality 2. Conversely, if voters' stock of information is not important, we should expect no difference across those who originally lived in municipality 1 and those who originally lived in municipality 2. Importantly, if we find a difference between the voters who originally lived in municipalities 1 and 2, we know that this difference cannot be attributable to the incumbent (who is the same) or the type of political system (which is also the same). As such, by analysing the electoral consequences of the jurisdictional reform process, we will be able to conduct a very "clean" test of whether voters' growing stock of information affects economic voting, and, in turn, find out whether it is plausible that this is the mechanism underlying the country-level and individual-level results.

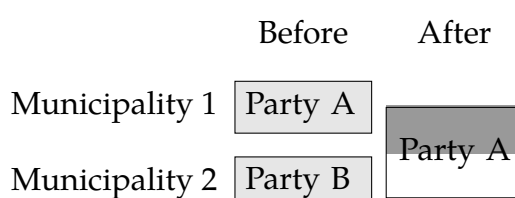


Figure 8.3 A stylized example of the consequences of the jurisdictional reform process. The shading denotes the electorate's stock of information about the incumbent mayoral party before the 2009 election.

Data and Model

To study the consequences of the jurisdictional reform, we examine election returns from the 2009 Danish municipal elections. In particular, we construct a dataset based on returns from 1,465 different precincts (i.e., polling places). Each precinct lies within one of 239 original municipalities (pre-reform) and 66 merged municipalities (post-reform).⁶ We do not use data from precincts which lie in municipalities that did not merge as a result of the reform, because these do not exhibit the type of within-municipality variation we are interested in, cf. figure 8.3.

In Danish municipalities mayors are not directly elected, but are instead appointed by a majority of the members in the city council. Often, this means a coalition of two or three ideologically similar parties decide to appoint a mayor from the largest

⁶ We collected this data from the Danish Election database. For details, see <http://valgdata.ps.au.dk/en/>.

party. Accordingly, the key dependent variable is change in electoral support for the incumbent mayoral party between the 2009 and 2005 city council elections (Δy).

The key independent variables are changes in the municipal unemployment rate from 2007 to 2009 ($\Delta unem$), and a dummy indicating whether the voters in the precinct had a different incumbent before and after the reform (*newinc*).⁷ Note that because all of the municipalities studied here merged with other municipalities in 2005, the variable *newinc* varies within the merged municipalities. Section S3 of the supplementary materials includes descriptive statistics on all variables.

Turning to modeling, we set the change in support for the mayoral party in each precinct as a linear function of whether voters had a new incumbent, changes in municipal unemployment levels, and an interaction between the two. We also include post-reform municipality fixed effects (θ), as well as a control for the level of support for the mayor at the last election (*lagy*). This leaves us with the following baseline model

$$\Delta y_{ij} = \beta_0 + \beta_1 newinc_{ij} + \beta_2 unem_j + \gamma newinc_{ij} \times unem_j + \beta_4 lagy_{ij} + \theta_j + \epsilon_{ij}, \quad (8.3)$$

where i indicates precinct and j indicates the post-reform municipality. The key estimate of interest is once again γ , which denotes the difference in the effect of the unemployment rate between those voters who have gotten a new incumbent and those who have not. We expect γ to be negative, so that increases in the unemployment rate has a larger negative effect if the voters in the precinct have gotten a new incumbent, and thus have less (prior) information about the incumbent. We include the municipality fixed effects, θ , to make sure that we are only comparing electorates which have the same incumbent (i.e., election returns from the same post-reform municipality). We also include support for the mayoral party in the last election ('05), because we want to take the mayoral party's baseline level of support in the different precincts into account.

Results

In the first column of table 8.3, we estimate the model presented in equation 8.3, using a maximum likelihood estimator to obtain municipality-clustered standard errors. Note that the baseline effect of increases in the municipal unemployment

⁷Because of the large redistricting reform, there is no comparable data on the level of unemployment in '05 or '06 at the municipal level, which is why we look at changes from '07 to '09.

rate is not estimated, because the baseline is perfectly collinear with the post-reform municipality fixed effect.

The key estimate of interest is the one attached to the interaction between increases in the unemployment rate and whether the incumbent is new to the electorate. Consistent with our expectations, the interaction estimate is negative and statistically significant. This suggests that increases in the unemployment rate have a larger impact on support for the incumbent mayoral party in precincts where voters have less experience with the incumbent mayor.

Table 8.3 Linear regression of change in support for the incumbent mayoral party

	(1)	(2)	(3)
New incumbent	1.92 (2.69)	1.91 (2.70)	1.44 (2.65)
Increase in unemployment rate × New incumbent	-2.13* (1.07)	-2.14* (1.08)	-2.08* (1.02)
Support for mayoral party 05	-0.63* (0.08)	-0.63* (0.07)	-0.63* (0.07)
Right wing mayor			-10.63* (0.95)
Proportion of votes for right wing parties			19.45* (7.69)
Turnout		5.83 (7.10)	-1.96 (5.89)
Log of eligible voters		-0.31 (0.36)	0.12 (0.30)
Municipality FE	✓	✓	✓
Observations	1,465	1,465	1,465

Standard errors in parentheses

Standard errors clustered by municipality.

+ $p < 0.10$, * $p < 0.05$

In figure 8.4, we illustrate this interaction effect by plotting the difference in support for the mayoral party between precincts where the voters have a lot of experience with the mayor (both pre- and post-reform) and precincts where the voters have little experience with the mayor (only post-reform) across increases in the unemployment rate.⁸ The figure shows that in municipalities where the unemployment rate did not increase, the mayoral party was just as popular in precincts where voters had little experience with the incumbent as in precincts where the voters had a lot of experience with the incumbent. However, in municipalities which saw the unemployment rate increase a lot, the mayoral party was way less popular among those who did not

⁸The 2009 elections were held just as the effects of the financial crisis were starting to kick in, and therefore the unemployment rate increased for all municipalities in this period.

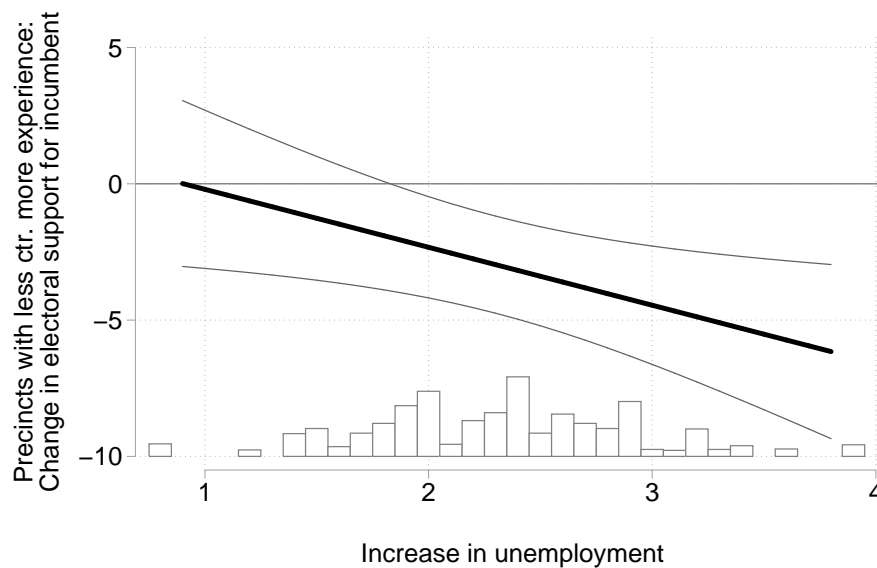


Figure 8.4 Differences in electoral support for the mayoral party between the precincts that had the same incumbent before and after the reform, and the precincts which did not, across changes in the municipal unemployment rate with 90 pct. confidence intervals. Derived from the model presented in column one of table 8.3. The bar plot shows the density of the variable increases in the unemployment rate.

know the mayoral party well. Put differently, those *without* a lot of prior information about the mayoral party seemed to be much more affected by recent increases in local levels of unemployment than those *with* a lot of prior information.

As for the country-level and the individual-level results, we examine whether these subnational results are sensitive to alternative specifications. In particular, we are interested in seeing whether some other characteristics of the precincts might explain the differences in economic voting between those who have experience with the incumbent mayor and those who do not. To control for the demographic characteristics of the precincts, we add controls for turnout and the size of the electorate in the second column of table 8.3. To control for the ideological make-up of the precincts, we add controls for whether the mayoral party is right-wing and for the proportion of voters who voted for a right-wing party in the third column. The inclusion of these controls does not affect the interaction estimate. It remains statistically significant, negative and of the same approximate size. In the supplementary materials, we also investigate the robustness of the results. In particular, we examine whether the interaction estimate is sensitivity to outliers in section S6, and whether

the interaction is robust to the checks suggested by Hainmueller, Mummolo and Xu (2016) in section S7.

These results tell us that voters' stock of information about the incumbent affect whether they 'vote economically'. As such, voters who have more time to get to know an incumbent, like those voters who had the same mayoral party both before and after the reform, are less likely to shift their support to or away from the incumbent based on how the economy is doing around election time. While this does not definitively show that voters growing stock of information is the only force driving down the economic vote as time in office increases, these findings make it more plausible that it is one such force.

Conclusion and Discussion

The British parliamentary elections of 1997 and 2001 featured two very different incumbents. One was the Conservative Party, in power for 18 years and headed by John Major, the prime Minister for seven years and cabinet member for the last 10. The other was the Labour party, in power for four years and headed by Tony Blair, prime minister with a new cabinet. As British voters searched for clues in 1997 and 2001 about the quality of the incumbent, some probably took a look at the economic situation. When these voters decided to what extent the economic situation should be leveraged in their eventual electoral decision, did the fact that these incumbents were so different matter? Did the fact that the incumbent up for election in 1997 had been in power for almost two decades make voters consider the economy differently than in 2001, when the incumbent had only been in power for four years? Answers to these questions are not easy to find in the existing literature on economic voting, which has generally paid little attention to how differences in incumbent tenure might moderate the economic vote.

In this article, we have tried to amend this by providing a thorough empirical investigation of the long term relationship between economic voting and time in office. Specifically, we have shown that the electoral support for executive parties becomes more independent of the economic situation as the parties time in office increase. This finding was arrived at using two markedly different datasets; one at the country-level and one at the individual-level; one using objective measures of economic conditions and one using a subjective measure.

To explain why the relationship between economic voting decreases as incumbents' time in office increases, we advanced a theoretical argument predicated on

Bayesian learning. It follows from Bayesian learning, that if voters have a lot of information about an incumbent, then their evaluation of this incumbent is less likely to be swayed by the economic situation around election time. Conversely, if voters have less information, they are willing to let the economic situation around election time count for more. Since voters naturally accumulate more information about the incumbent as time in office increases, the economic situation should matter less for voters' evaluation of the incumbent, effectively driving down economic voting. Returning to the British case, our study thus suggests that voters relied more on recent economic conditions when evaluating the relatively new Labour administration than when evaluating the relatively old Conservative administration, because British voters' had come to know the Conservative party quite well, whereas voters had less to go on, apart from the economic situation, when evaluating the Labour incumbent.

In order to examine the empirical implications of this argument in more detail, we conducted an additional study of Danish municipal elections. In particular, we studied the level of economic voting following a large redistricting reform, which created within-municipality differences in the amount of experience the electorate had with the same incumbent mayoral party. In line with our theoretical argument, we found that voters who had less experience with an incumbent were more likely to hold the incumbent accountable for local levels of unemployment.

Our findings are noteworthy for at least two reasons. First, incumbents are likely to be more responsible for the quality of economic conditions as their time in office increases, and accordingly, following the large literature on clarity of responsibility, we should expect incumbents to be held more accountable for their economic performance as their time in office increases, not less. However, our results suggest that other factors, like Bayesian learning, are more important than any potential changes in clarity of responsibility which occur as time in office increases. Second, previous studies have found that incumbent support becomes more dependent on recent economic conditions in the first few years of an incumbent's time in office. Our study does not find any signs of this – instead, we consistently find that the importance of recent economic conditions decreases with time in office.

This article has examined a large subset of countries and elections, increasing the generalisability of the results. Even so, the results have mainly focused on advanced democracies, delimiting the scope of inference to this type of countries. A consequence of primarily studying advanced democracies is that one tends to examine more stable political systems. In the present context, this translates into studying incumbents who have, relatively speaking, served for a long time. This

might, in part, explain why our findings diverge from those in previous research. As mentioned earlier in the article, the most extensive exploration of the link between tenure and economic voting in the existing literature focused exclusively on Latin America (Singer and Carlin, 2013); a more politically volatile region. Here, researchers found that retrospective economic voting increased markedly in the first two years of the incumbent's time in office. They were able to detect this short term shift in the economic vote because half of the incumbents they examined had been in office for less than 2.5 years. In comparison, only one fifth of incumbents in our country-level dataset had been office for less than three years (one in three for the individual-level dataset). Based on the difference in coverage for the time in office variable, one can imagine that the Latin America study was at least half right – economic voting might shoot up in the first two years of the incumbents term, but then depreciate in the long term (cf. our findings). Our data, however, is not detailed enough to test whether the relationship between the economic vote and time in office follows such a curvilinear pattern. Instead, we simply conclude that in the long run, incumbent tenure crowds out economic voting.

Future studies should also try to come closer to a definitive understanding of why incumbent tenure tends to crowd out economic voting. Bayesian learning seems to be part of the explanation, however there might be other factors at work as well, such as changing priorities among voters or changing patterns of partisanship. Future research might be able to uncover these mechanisms using controlled survey or lab experiments (for work along these lines, see Mitchell, 2012). Future research could also delve into which factors become more important as the explanatory power of the economy declines. Do voters, for instance, become more interested in the ideological position of the executive party and/or their specific policies as tenure increases, or will electorates simply become less malleable, in general, as their incumbent's time in office increase.

Supplementary materials

S1: A Formalisation of the Bayesian Learning Model

In this model we examine a set of voters who have to decide whether to re-elect an incumbent. We assume that the voters are more likely to re-elect the incumbent if voters believe the incumbent is more competent. Given this assumption, the voters' goal is to construct a set of rational beliefs about the incumbent's competence given the available evidence. Specifically, imagine an incumbent I which was elected at $t = 0$, and who is now up for re-election at $t = 1$. We denote the incumbent's competence as C_I . Based on the voters' prior experiences with other incumbents, they start off with a normally distributed prior belief about C_I , which we standardise to have a mean of zero with a variance of 1.

Since voters are interested in the incumbent's competence, C_I , the voters try to infer how competent the politician is based on the economic situation, y , which is affected by C_I . However, the economy is also affected by a non-competence related shock ϵ , which is independently and identically drawn in each period from a normal distribution with mean zero and variance σ_ϵ^2 . Accordingly, the economic situation at $t = 1$ can be defined as:

$$y_1 = C_I + \epsilon_1 \quad (8.4)$$

In this equation, voters only observe y_1 ; however, voters know the distribution the non-competence related shock is drawn from. As such, voters face a signal extraction problem, which can be solved by using the Bayes rule to update their prior beliefs about C_I using y_1 . This leaves voters with the following posterior beliefs about C_I :

$$C_I|y \sim N\left(\frac{1}{\sigma_\epsilon^2 + 1}y_1; \frac{\sigma_\epsilon^2}{\sigma_\epsilon^2 + 1}\right) \quad (8.5)$$

Equation 8.5 tells us that voters' beliefs about the incumbent's expected competence are improving in y_1 . That is, a better economic situation leads the voter to infer that the incumbent is likely to be more competent. Specifically, the effect of a one unit increase in y_1 on expected competence is $\frac{1}{1+\sigma_\epsilon^2}$.

From this, we can also see that effect of the economic situation on beliefs about competence becomes smaller as σ_ϵ^2 becomes larger. That is, as the variation in non-competence related shocks to the economy increases, it becomes more likely that any variation in the economic situation is due to non-competence related shocks,

and accordingly the economy becomes a less efficient estimator of the incumbent's competence. In effect, σ_ϵ^2 can be thought of as being an inverse measure of clarity of responsibility. If σ_ϵ^2 is large, the incumbent is probably not responsible for changes in economic conditions, if σ_ϵ^2 is small the incumbent is probably responsible for changes in economic conditions.⁹

Imagine the incumbent is re-elected in $t = 1$. In period $t = 2$, the voters have to decide once again whether to vote for the incumbent. However, now the voters' prior beliefs about the incumbent incorporate the information obtained about C_I at $t = 1$. That is, voters' prior beliefs now have a mean of $\frac{1}{1+\sigma_\epsilon^2}y_1$ and a variance of $\frac{\sigma_\epsilon^2}{\sigma_\epsilon^2+1}$. Note that the variance of the new prior is smaller than the original prior, since $1 > \frac{\sigma_\epsilon^2}{\sigma_\epsilon^2+1}$ for all possible values of σ_ϵ^2 (i.e., voters know more about the incumbent than they did before).

Voters update their prior beliefs using Bayes rule, based on the economic situation in $t = 2$, y_2 , which is equal to:

$$y_2 = C_I + \epsilon_2 \quad (8.6)$$

This leaves the voter with the following posterior beliefs about the incumbent's competence.

$$C_I|y_2, y_1 \sim N\left(\frac{1}{(\sigma_\epsilon^2 + 2)}y_1 + \frac{1}{(\sigma_\epsilon^2 + 2)}y_2; \frac{\sigma_\epsilon^2}{\sigma_\epsilon^2 + 2}\right) \quad (8.7)$$

Just as in period 1, a better economic situation in period 2, y_2 , is used to infer that incumbent competence is higher, and just like in period 1, σ_ϵ^2 attenuates the degree to which voters can use the economic situation to make inferences about C_I . However, there is one key difference from period 1: the effect of the economic situation on voters' expectations about the incumbent's competence have decreased.

In period 1 the effect of a one unit increase in y_1 was $\frac{1}{1+\sigma_\epsilon^2}$. In period 2 the effect of a one unit increase in y_2 is $\frac{1}{2+\sigma_\epsilon^2}$. Since $\frac{1}{1+\sigma_\epsilon^2}$ is larger than $\frac{1}{2+\sigma_\epsilon^2}$, variation in the economic situation has less bearing on how competent voters expect the incumbent to be when the incumbent is up for re-election the second time. Economic voting decreases with time in office. This is a key result from the model, which underlines the assertion made in the theoretical discussion of the main article: as voters' information about the incumbent accumulate, the recent economic situation comes to play a smaller role in shaping voters' beliefs about the incumbent.

⁹This conclusion closely mirrors the one found by Duch and Stevenson (2008). Using a slightly more complicated set-up, they show that as control of economic conditions becomes more independent of elected officials (i.e. the size of non-competence related shocks increase), voters beliefs about the incumbent's competence depend less on economic conditions.

Increasing Clarity of Responsibility Versus Bayesian Learning

In the model presented above, we assumed that the economic situation in period 1 and the economic situation in period 2 was a result of the same mix of competence and non-competence related shocks. Some previous literature on the relationship between economic voting and time in office makes a different assumption (e.g., Nadeau, Niemi and Yoshinaka, 2002). In particular, these researchers assume that the incumbent becomes more responsible for the economic conditions as their time in office increases. In the terminology of our model, they think incumbent competence becomes more important relative to non-competence related shocks at $t = 2$. What happens if we incorporate this alternative assumption into our model?

We introduce the assumption by letting our inverse measure of clarity of responsibility, σ_ϵ^2 , decrease with time in office. In particular, we assume that the variance decreases from σ_ϵ^2 at $t = 1$ to $\tilde{\sigma}_\epsilon^2$ at $t = 2$, where $\tilde{\sigma}_\epsilon^2 < \sigma_\epsilon^2$. We denote the rate at which the variance decreases as α , where $\alpha = \tilde{\sigma}_\epsilon^2 / \sigma_\epsilon^2$. If α is close to 1, there is only a small decrease in the variation of the error term, signifying that incumbents become only slightly more responsible as their time in office increase; if α is close to 0 there is a marked decrease in the variation of the error term, signifying that incumbents become a lot more responsible as their time in office increases. Effectively, α is therefore negatively related to the increase in the clarity of responsibility over time.

How does introducing this assumption affect voters' beliefs about incumbent competence? In the first period, nothing changes, because the assumption only alters the variance of the non-competence related shocks in period 2. However, in period 2, voters take into account that the variance in ϵ has decreased to $\tilde{\sigma}_\epsilon^2$, and consequently rely more on the competence signal relayed by the economic situation y_2 . In particular, after updating their priors using Bayes rule, voters' posterior beliefs about incumbent competence can be described as follows:

$$C_I|y_2, y_1 \sim N\left(\frac{1}{\frac{1}{\alpha} + \sigma_\epsilon^2 + 1}y_1 + \frac{1}{1 + (\sigma_\epsilon^2 + 1)\alpha}y_2; \frac{\tilde{\sigma}_\epsilon^2\sigma_\epsilon^2}{\tilde{\sigma}_\epsilon^2\sigma_\epsilon^2 + \tilde{\sigma}_\epsilon^2 + \sigma_\epsilon^2}\right) \quad (8.8)$$

Note that in equation 8.8, the extent to which voters rely on y_2 depends negatively on α , which we defined as the rate at which the variance in the non-competence related shocks decreases from $t = 1$ to $t = 2$. This makes intuitive sense, because a large decrease in variance corresponds to a large *increase* in the clarity of political responsibility over time. As such, if clarity of responsibility increases a lot with time in office, α is low, and voters tend to rely more on y_2 .

Just as we did above, we can compare the extent of economic voting across time in office by comparing the effect of a one unit increase in y_2 on C_I at $t = 2$ (cf. equation 8.8) with the effect of a comparable increase in y_1 on C_I at $t = 1$ (cf. equation 8.5) (i.e., we compare $\frac{dC_I}{dy_1}$ and $\frac{dC_I}{dy_2}$). If the effect of y_1 is larger than the effect of y_2 , then economic voting decreases over time; if the effect of y_2 is larger than the effect of y_1 , then economic voting increases over time. This inequality can be written as:

$$\frac{dC_I}{dy_2} < \frac{dC_I}{dy_1} \iff \frac{1}{1 + (\sigma_\epsilon^2 + 1)\alpha} < \frac{1}{1 + \sigma_\epsilon^2} \quad (8.9)$$

which can be simplified to

$$\sigma_\epsilon^2 \left(\frac{1}{\alpha} - 1 \right) < 1 \quad (8.10)$$

If the inequality in equation 8.10 is satisfied, economic voting decreases with time in office. When will this inequality be satisfied? All other things being equal, it is more likely to be satisfied if the increase in the clarity of responsibility is low (i.e. α close to 1), and, other things being equal, is also more likely to be satisfied if the overall role placed by non-competence related shocks is low (σ_ϵ^2 is small). Note that if α is 1, signifying no increase in the clarity of responsibility over time, the condition in equation 8.10 will always be satisfied, and incumbent tenure will always crowd out economic voting.

In sum, it is not possible to form unambiguous theoretical expectations for how incumbent tenure and economic voting are related based on this augmented model. As such, whether or not incumbent tenure does in fact crowd out economic voting, will depend on the exact beliefs voters hold about α and σ_ϵ^2 . What it does tell us, however, is that there are countervailing forces at work as incumbent tenure increases.

S2: Description of the Samples

The samples used in the country-level and individual-level data are described in tables S.8.1 and S.8.2. In table S.8.3, we show the full distribution of the tenure variables in both the country-level and individual-level data. For comparison, we also include the tenure variable from the Latinobarómetro data, which is what Singer and Carlin (2013) use for their study.

Table S.8.1 Elections included in the country-level analysis

	Minimum	Maximum	Number of Elections
Argentina	1985	2001	12
Australia	1961	2007	19
Austria	1971	2008	12
Belgium	1961	2007	15
Bolivia	1989	2002	8
Brazil	1990	2002	7
Bulgaria	1991	2001	6
Canada	1962	2008	16
Chile	1993	2001	5
Colombia	1982	2002	12
Costa Rica	1982	2002	12
Denmark	1964	2007	18
Dominican Republic	1990	2002	6
Ecuador	1984	1998	11
El Salvador	1985	2000	9
Finland	1962	2007	17
France	1968	2007	14
Germany	1972	2009	11
Greece	1981	2009	9
Honduras	1989	2001	8
Iceland	1963	2007	13
India	1980	1998	6
Ireland	1973	2007	10
Israel	1969	2006	12
Italy	1972	2008	10
Luxembourg	1979	2009	7
Madagascar	1996	2001	2
Netherlands	1963	2006	14
New Zealand	1978	2008	11
Norway	1969	2009	11
Papua New Guinea	1987	2002	4
Peru	1990	2001	6
Poland	1993	2001	3
Portugal	1980	2009	10
Spain	1979	2008	9
Sweden	1976	2006	10
Switzerland	1983	1999	5
Trinidad and Tobago	1991	2000	3
Turkey	1987	2002	5
United Kingdom	1964	2010	12
United States	1978	2002	19
Total	1961	2010	409

Table S.8.2 Observations included in the individual-level analysis

	1989	1994	1999	2004	2009	2014	Total
Denmark	832	1642	759	999	867	1063	6162
France	749	1321	363	1034	513	1055	5035
Germany	875	1333	777	381	691	1610	5667
Greece	525	1236	320	373	689	1080	4223
Ireland	752	1351	370	892	762	1074	5201
Italy	673	960	2446	1151	561	1068	6859
Netherlands	871	1610	804	1260	802	1087	6434
Portugal	521	1154	259	605	623	1020	4182
Spain	618	1216	581	887	680	1097	5079
United Kingdom	856	1603	711	1104	690	1378	6342
Total	7272	13426	7390	8686	6878	11532	55184

Table S.8.3 Distribution of Time in Office (years)

	Country-level	Individual-level	Singer & Carlin (2013)
Less than six months			11.8 (25)
1	5.4 (22)	13.3 (8)	18.9 (40)
2	9.5 (39)	16.7 (10)	18.9 (40)
3	8.8 (36)	15.0 (9)	17.0 (36)
4	25.2 (103)	10.0 (6)	16.5 (35)
5	12.2 (50)	5.0 (3)	7.5 (16)
6	6.6 (27)	6.7 (4)	3.3 (7)
7	3.9 (16)	11.7 (7)	2.8 (6)
8	9.3 (38)	3.3 (2)	1.4 (3)
9	2.9 (12)	3.3 (2)	0.5 (1)
10	3.2 (13)	1.7 (1)	0.9 (2)
11	2.4 (10)	1.7 (1)	0.5 (1)
12	3.2 (13)	8.3 (5)	
13	2.2 (9)		
14	1.0 (4)		
15	0.5 (2)	1.7 (1)	
16	0.5 (2)		
17	0.7 (3)	1.7 (1)	
18	0.5 (2)		
20	0.5 (2)		
21	0.5 (2)		
25	0.7 (3)		
30	0.2 (1)		
Total	100.0 (409)	100.0 (60)	100.0 (212)

Number of country-level observations in parentheses.

S3: Variable Descriptions and Descriptive Statistics

Descriptive statistics for the country-level data are presented in table S.8.4.

Table S.8.4 Descriptive statistics

	Mean	SD	Min	Max	n
Electoral support for incumbent party	33.63	12.26	0.00	59.20	433
Effective number of parties	3.87	1.63	1.18	10.49	415
Election year	1988.82	11.82	1961.00	2010.00	433
Electoral support for incumbent party (initial election)	37.99	12.61	0.00	67.30	433
Executive election	0.14	0.35	0.00	1.00	433
Economic growth - 2 years	3.08	2.65	-8.78	14.90	428
Coalition partners	1.53	1.28	0.00	3.00	433
Government has majority in legislature	0.71	0.46	0.00	1.00	348
Number of elections	12.57	4.06	5.00	19.00	431
Years pr. term	2.34	1.03	0.00	4.67	433
Fixed term	0.15	0.36	0.00	1.00	433
Election year economic growth	2.91	3.14	-11.70	13.85	433
Change in electoral support	-4.36	8.03	-42.80	20.70	433
Tenure	6.02	4.25	1.00	30.00	409
Trichotomised tenure	0.70	0.77	0.00	2.00	409
Mismatch tenure (person v. party)	0.35	0.48	0.00	1.00	433

The question wording for the different questions used in this individual-level analysis are as follows.

- Executive party vote: “If there were a general election tomorrow, which party would you vote for?” Executive parties are coded 1, others are coded 0.
- Executive party vote (last election): “Which party did you vote for at the General Election of [Year]?” Same coding as for the vote variable.
- Ideology: “In political matters people talk about ‘the left’ and ‘the right.’ What is your position? Please indicate your views using any number on a 10-point scale. On this scale, where 1 means ‘left’ and 10 means ‘right’, which number best describes your position?”
- Class: “If you were asked to choose one of these five names for your social class, which would you say you belong to — the working class, the lower middle class, the middle class, the upper middle class, or the upper class?”
- Religiosity: “How often do you attend religious services: several times a week, once a week, a few times a year, once a year or less, or never?”

- National economic perceptions (NEP): In 1989, 1994, 2004, 2009 and 2014: “What do you think about the economy? Compared to 12 months ago, do you think that the general economic situation in this country is: a lot better, a little better, stayed the same, a little worse, or a lot worse?” In 1999: “How about the state of the [country’s] economy? Very satisfied, somewhat satisfied, somewhat dissatisfied, very dissatisfied?”

Descriptive statistics for the individual-level data are presented in table S.8.5.

Table S.8.5 Descriptive statistics

	Mean	SD	Min	Max	n
Ideology	0.50	0.26	0.00	1.00	51030
Year	2001.63	8.70	1989.00	2014.00	55184
National Economic Perceptions	0.43	0.28	0.00	1.00	55184
Class	0.45	0.31	-0.33	1.33	52700
Religiosity	0.51	0.32	0.00	1.17	49165
Prospective NEP	0.53	0.40	0.00	3.50	39571
Country Code	5.53	2.90	1.00	10.00	55184
Economic growth	1.85	2.96	-5.64	10.76	48842
Inflation	2.24	2.82	-4.48	13.70	47967
Unemployment rate	10.16	5.24	3.40	26.30	48842
Coalition government	0.63	0.48	0.00	1.00	55184
Lagged executive party vote	0.34	0.47	0.00	1.00	49266
Executive party vote	0.29	0.46	0.00	1.00	55184
Tenure	5.46	4.24	1.00	17.00	55184
Trichotomised time	0.78	0.82	0.00	2.00	55184

Descriptive statistics for the subnational data are presented in table S.8.6.

Table S.8.6 Descriptive statistics

	Mean	SD	Min	Max	n
Total number of eligible voters	2349.64	2563.93	6.30	31458.61	1823
Support for mayoral party 05	35.11	18.34	0.00	87.74	1823
Proportion of votes cast for mayoral party 09	32.26	14.52	0.00	82.61	1823
Municipality amalgamated	0.80	0.40	0.00	1.00	1823
New incumbent	0.44	0.50	0.00	1.00	1823
Change in support for mayoral party	-2.85	15.67	-45.20	78.15	1823
Unemployment 07	2.31	0.86	1.00	8.70	1823
Unemployment 09	4.51	0.81	2.30	8.30	1823
Right wing mayor	0.43	0.50	0.00	1.00	1823
Turnout	0.69	0.06	0.37	0.89	1823
Proportion of votes for right wing parties	0.48	0.14	0.10	0.88	1823
Increase in unemployment rate	2.20	0.67	-0.40	4.10	1823
Log of eligible voters	7.24	1.10	1.84	10.36	1823
Trichotomized unemployment	1.11	0.80	0.00	2.00	1465

S4: Alternative Measure of Growth

In table S.8.7, we re-estimate the models from table 8.1 using an alternative measure of economic growth: economic growth across the past two years, rather than just the past year. The interaction effect becomes slightly larger, remains negative and statistically significant.

Table S.8.7 Linear regression of changes in executive party vote share

	(1)	(2)	(3)	(4)
Economic growth - 2 years	0.93*	1.09*	1.05*	1.31*
	(0.29)	(0.35)	(0.39)	(0.38)
Tenure	-0.01	0.02	-0.22	-0.19
	(0.14)	(0.13)	(0.16)	(0.18)
Economic growth - 2 years × Tenure	-0.08*	-0.10*	-0.08*	-0.08*
	(0.03)	(0.03)	(0.03)	(0.03)
Executive election	-2.98*	-2.04	-1.08	-1.99*
	(1.52)	(1.51)	(1.94)	(0.55)
Year FE		✓	✓	
Country FE			✓	✓
Leader FE				✓
Observations	406	406	406	406

Standard errors in parentheses

Standard errors clustered by country.

+ $p < 0.10$, * $p < 0.05$

S5: Using Controls in the Country-level Data

Below, we add some controls to the models estimated on the country-level dataset of elections. This means dropping a number of the observations, about 80, for which we do not have data coverage for the control variables. In order to make the estimates with and without controls more comparable, we start by estimating the same models as in table 8.1 on the smaller sample of elections, for which we have controls. This is done in the four furthest left columns of table S.8.8. As we can see, the results are fairly similar to those found using the full sample. The main difference is that the interaction effects become slightly smaller, and the standard errors become slightly larger, leaving the interaction terms insignificant.

Next, we introduce the controls. The controls we use are number of government coalition partners, including a dummy for one, two and three or more partners; majority government, including a dummy for whether the government has more than fifty percent of the seats in parliament; and effective number of parties in parliament, a linear index measuring the size-adjusted number of parties in parliament. All these variables have been taken from the database of political institutions (Beck et al., 2001). They have been chosen with the following considerations in mind: we know that government composition affects economic voting (cf. Powell and Whitten, 1993), depressing the clarity of responsibility for economic policy, and it seems plausible that the effective number of parties can work in a similar way – the more parties, the more political actors there are to blame for any economic misfortune. It also seems likely that government and parliamentary composition can influence the tenure of the executive party, making it a good candidate for a confounding factor. Finally, unlike most other institutional factors, government and parliamentary composition are not already controlled for using the year, country and leader fixed effects. In the last four columns of table S.8.8 the controls are introduced. This leaves the interaction effects practically unchanged.

In sum, while the interaction estimates remain substantially unchanged, the statistical significance of the interaction coefficients drops when introducing the controls; however, this is only a result of the fact that another, smaller sample of elections are being analysed. As such, there is no evidence that the controls introduced in any way confound the negative relationship between economic voting and time in office.

Table S.8.8 Linear regression of changes in executive party vote share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Executive election	-3.00 ⁺ (1.57)	-1.88 (1.54)	-1.20 (1.99)	-4.52* (1.35)	-2.21 (1.61)	-1.00 (1.54)	-0.20 (1.93)	-3.38* (1.29)
Economic growth	0.68* (0.28)	0.71* (0.30)	0.60 ⁺ (0.32)	1.08* (0.34)	0.75* (0.28)	0.79* (0.30)	0.67* (0.31)	1.15* (0.31)
Tenure	-0.08 (0.16)	-0.04 (0.14)	-0.38 ⁺ (0.20)	-0.27 (0.21)	-0.05 (0.15)	-0.01 (0.14)	-0.36 ⁺ (0.19)	-0.24 (0.21)
Economic growth × Tenure	-0.05 (0.03)	-0.05 (0.04)	-0.03 (0.04)	-0.05 (0.04)	-0.05 (0.03)	-0.05 (0.04)	-0.03 (0.04)	-0.06 (0.04)
Government has majority in legislature					-2.28* (1.12)	-2.69* (1.30)	-2.62 ⁺ (1.40)	-4.51* (1.82)
One coalition partner					2.59* (1.30)	2.62 ⁺ (1.44)	1.02 (1.57)	0.41 (2.58)
Two coalition partners					-0.41 (1.83)	-0.19 (1.71)	-0.58 (1.62)	-0.73 (2.18)
More than two coalition partners					1.44 (1.43)	1.10 (1.67)	-0.31 (1.76)	2.68 (2.08)
Effective number of parties					0.52 (0.41)	0.54 (0.49)	1.09 (0.68)	0.48 (0.58)
Time FE		✓	✓			✓	✓	
Country FE			✓	✓			✓	✓
Leader FE				✓				✓
Observations	330	330	330	330	330	330	330	330

Standard errors in parentheses

Standard errors clustered by country.

⁺ $p < 0.10$, * $p < 0.05$

S6: Sensitivity to Outliers

Are the interaction effects presented above based on broad patterns in voting behavior or idiosyncrasies related to just one country? This is always an important question when dealing with time-series cross-sectional data. In order to investigate whether this was the case for the present analyses, we re-estimated the key models in the country-level, individual-level and subnational datasets, looking for evidence of instability in the effect-sizes which stem from the exclusion of one important set of cases.

For the country-level data, we re-estimate the models from table 8.1 excluding one country, for all countries in the sample. The resulting 41×4 regression coefficients, attached to the interaction between economic conditions and incumbent tenure, are plotted for each model in the top left panel of figure S.8.1. As can be seen from this figure, the interaction coefficients in models 1 and 2 seem rather stable; however, in models 3 and 4 one of the estimated coefficients deviates substantially from the rest. An inspection of the underlying data, reveals that the omitted country in this context is Luxembourg. There are two reasons why this is not that disconcerting. First, the problem is less severe in model 4, which is the more demanding model of the two. Second, Luxembourg is not one of the countries included in the individual-level dataset, and therefore the negative relationship between economic voting and tenure cannot be attributable to Luxembourg alone.

For the individual-level data, we re-estimate the models from table 8.2 excluding one survey, for all surveys in the sample (i.e. country-year). The resulting 60×3 logistic regression coefficients, attached to the interaction between economic perceptions and incumbent tenure, are plotted for each model in the top right panel of figure S.8.1. As can be seen from this figure, the interaction coefficients are relatively stable across all models.

For the subnational data, we re-estimate the models from table 8.3 excluding one municipality, for all municipalities in the sample. The resulting 66×3 regression coefficients, attached to the interaction between *unem* and *newinc*, are plotted for each model in the bottom panel of figure S.8.1. As can be seen from this figure, the interaction coefficients are relatively stable across all models.

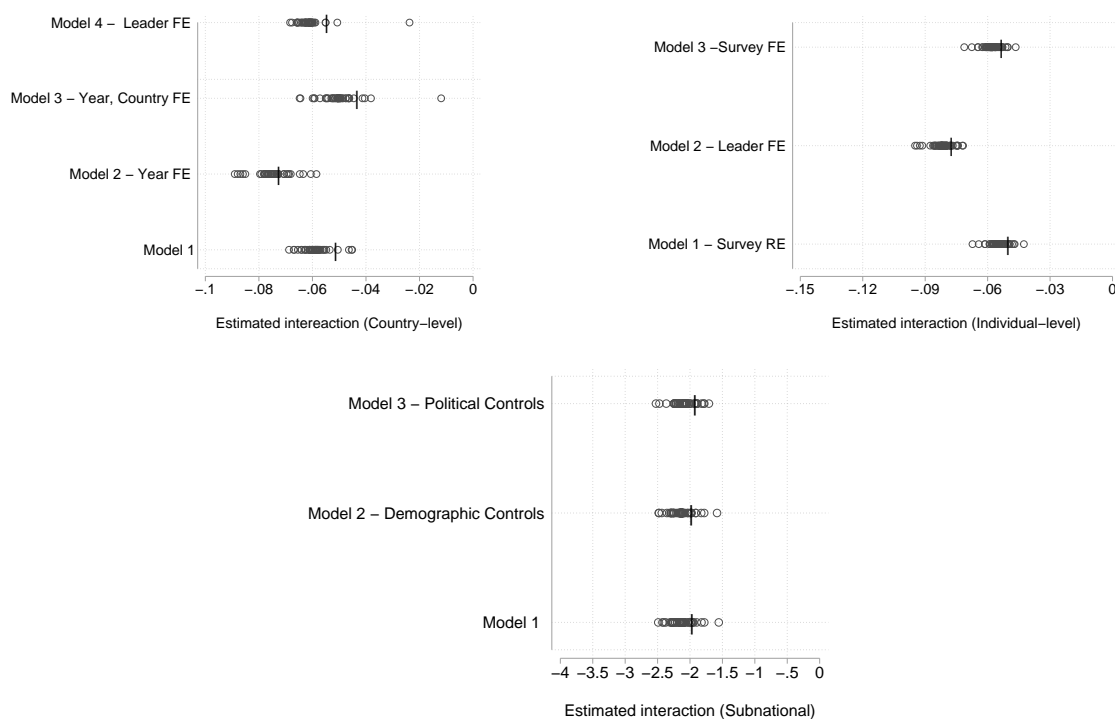


Figure S.8.1 Lines represent the interaction coefficients from linear and logit models in tables 8.1, 8.2 and 8.3. Each dot in the top left panel represents an interaction coefficient from one of the four country-level linear models, estimated with one of the 41 countries omitted. Each dot in the top right panel represents an interaction coefficient from one of the three individual-level logit models, estimated with one of the 60 surveys omitted. Each dot in the bottom panel represents an interaction coefficient from the three subnational linear models, estimated with one of the 66 municipalities omitted.

S7: Further Checks of the Interaction Terms

In a recent paper, Hainmueller, Mummolo and Xu (2016) suggests three diagnostics to run when encountering a multiplicative interaction term. Below, we look at each of these in turn for the interactions estimated in the country-level, individual-level and subnational data.

The first diagnostic is examining whether the L-kurtosis of the interaction variable is below 0.16. If the L-kurtosis is above 0.16, then much of the variation in the interaction variable is based on just a few observations. The L-kurtosis for the time in office variable in the country-level dataset is 0.2. This means that the interaction effect in the country-level data potentially relies on just a few observations, making the interaction term less reliable. The L-kurtosis for the time in office variable in the individual-level dataset is 0.075. In the subnational data, the L-kurtosis for the unemployment variable is 0.14. This is below the cut-off, and accordingly, we probably do not need to be concerned with the reliability of the interaction variable in the individual-level or the subnational data.

The second diagnostic looks for monotonicity in the average marginal effects. That is, we should expect average marginal effects to move monotonically with the interaction variable. To test this we trichotomised our interaction variables for all three datasets based on the variables' terciles. For the linear interaction models we then estimate the average marginal effect at the median of each tercile using a binning estimator (cf. equation (4) in Hainmueller, Mummolo and Xu, 2016). For the non-linear model which analyze the individual-level data we cannot use the binning estimator. Instead, we estimate a model using the trichotomised interaction variable as a set of dummy-interactions instead of the linear interaction, deriving the average marginal effects for the bottom, middle and top tercile. For the country-level, individual-level, and subnational data, we find that the average marginal effects monotonically decrease across the three terciles.

The final diagnostic is examining the linearity of the interaction. To do this, we plot the average marginal effects from the trichotomised interaction terms, along with the average marginal effects derived from simple linear interaction terms, in figure S.8.2. The trichotomised interaction terms are plotted at the median within each tercile. While the average marginal effects from the trichotomised interaction terms do not match the average marginal effect from the linear interaction terms exactly, they do not deviate substantially from each other.

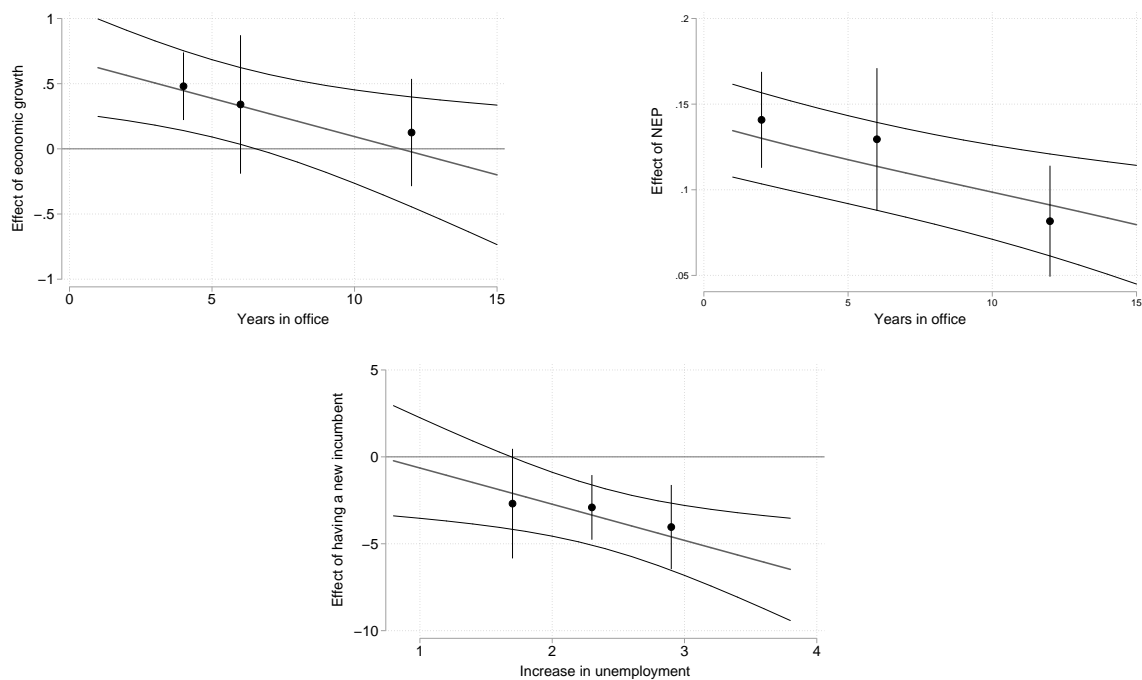


Figure S.8.2 The lines represent the average marginal effects of economic growth, national economic perceptions and *newinc* across the interaction variable. Derived from column one of table 8.1, column three of table 8.2 and column one of table 8.3. Dots represent the average marginal effects from binning estimators, which include a trichotomised interaction. All the average marginal effects are plotted with 90 pct. confidence intervals.

S8: Strategic Election Timing

To probe the plausibility of the strategic election timing explanation, we augment our country-level analysis in two different ways.

First, we introduce a control variable which measures how often an incumbent calls for an election (i.e. years served divided by elections called). By introducing this variable we hold constant the incumbents' inclination to call early elections. We add this variable as a control to the set of models already estimated in table 8.1, and report estimates of these extended models in the first four columns of table S.8.9. As can be seen from table S.8.9, the interaction remains negative, it has the same size, and in three of the four specifications, it is statistically significant ($p < 0.1$).

Table S.8.9 Linear regression of changes in executive party vote share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Economic growth	0.68*	0.79*	0.63*	0.90*	0.86 ⁺	0.47*	-0.19	0.71
	(0.25)	(0.28)	(0.29)	(0.30)	(0.45)	(0.15)	(0.41)	(0.54)
Tenure	-0.08	-0.06	-0.31 ⁺	-0.25	0.23*	-0.02	-0.40*	0.04
	(0.15)	(0.14)	(0.16)	(0.17)	(0.03)	(0.08)	(0.17)	(0.08)
Economic growth × Tenure	-0.06 ⁺	-0.08*	-0.05	-0.06*	-0.09*	0.01	-0.02	-0.10*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
Executive election	-3.01 ⁺	-2.06	-1.31	13.80*	-3.03	-1.76	-0.61	
	(1.55)	(1.53)	(1.94)	(1.73)	(3.86)	(2.48)	(3.29)	
Time FE		✓	✓			✓	✓	
Country FE			✓	✓			✓	✓
Leader FE				✓				✓
Observations	409	409	409	409	60	60	60	60

Standard errors in parentheses

Standard errors clustered by country.

Executive election dummy omitted in column (8) due to perfect collinearity with Leader FE.

⁺ $p < 0.10$, * $p < 0.05$

Second, we disentangle election-timing and tenure by restricting the sample of elections to the five countries in our dataset where terms are fixed. This leaves 60 of the original 409 elections. In these countries, the executive cannot time the election, and accordingly, any relationship found between time in office and the importance of the economy cannot be attributed to election timing. Using this restricted sample, we re-estimate the models from table 8.1. The key estimates from these models are reported in the four rightmost columns of table S.8.9. As can be seen from table S.8.9, the interaction effect remains negative and statistically significant in the most demanding model, which includes the leader fixed effects.

Taken together, the fact that, across both types of control for election timing, the interaction remains negative and substantially unchanged in the most demanding

specification (cf. columns 4 and 8), suggests that the results laid out in table 8.1 were not the result of strategic election-timing.

S9: Coalition and Single-party Governments

In table S.8.10, we re-estimate the models from table 8.1 only for single-party governments (columns one through four) and only for coalition governments (columns five through eight). Across all specifications the estimated interaction coefficient is negative, however, there are some differences across the two sets of models. In the models with no controls and the model with leader fixed effects, the negative interaction seems to be smaller for single-party governments. In the models with year and country fixed effects, the interaction seem to be smaller for multi-party governments. As such, there are no consistent differences across the two groups.

This suggests that the negative interaction term identified in the country-level data cannot be explained in terms of differences in how voters judge coalition and single-party governments over time. If this was the case, we would expect to see no interaction between time in office and economic voting for the single-party governments, and a very strong and statistically significant interaction among coalition governments. This is not what we find in table S.8.10.

Table S.8.10 Linear regression of changes in executive party vote share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Economic growth	0.70 ⁺	1.04 ⁺	0.75	0.98	0.45 ⁺	0.61*	0.62*	0.21
	(0.36)	(0.53)	(0.67)	(0.71)	(0.25)	(0.26)	(0.21)	(0.28)
Tenure	-0.37*	-0.05	-0.28	-0.64 ⁺	0.01	-0.06	-0.25	-0.23
	(0.19)	(0.30)	(0.31)	(0.35)	(0.15)	(0.16)	(0.16)	(0.16)
Economic growth × Tenure	-0.03	-0.09	-0.09	-0.01	-0.07*	-0.07 ⁺	-0.06*	-0.04
	(0.04)	(0.07)	(0.08)	(0.10)	(0.03)	(0.04)	(0.03)	(0.03)
Time FE		✓	✓			✓	✓	
Country FE			✓	✓			✓	✓
Leader FE				✓				✓
Observations	113	113	113	113	239	239	239	239

Standard errors in parentheses

Standard errors clustered by country.

⁺ $p < 0.10$, * $p < 0.05$

In table S.8.11, we re-estimate the models from table 8.2 only for single-party governments (columns one through three) and only for multi-party governments (columns three through six). There are no substantial differences across the two sets of models, which once again suggest that any differences in economic voting across tenure cannot be explained away by differences in how voters hold single-party and coalition governments electorally accountable for the economy.

Table S.8.11 Multi-level logit model of voting for executive party

	(1)	(2)	(3)	(4)	(5)	(6)
National Economic Perceptions	2.44*	2.48*	2.44*	1.56*	1.60*	1.59*
	(0.25)	(0.26)	(0.26)	(0.23)	(0.22)	(0.22)
Tenure	0.03	-0.02		0.06 ⁺	-0.02	
	(0.04)	(0.04)		(0.03)	(0.06)	
National Economic Perceptions × Tenure	-0.06 ⁺	-0.07 ⁺	-0.06 ⁺	-0.07*	-0.08*	-0.07*
	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
Individual lvl. controls	✓	✓	✓	✓	✓	✓
Survey RE	✓	✓	✓	✓	✓	✓
Leader FE		✓	✓		✓	✓
Survey FE			✓			✓
Observations	14,444	14,444	14,444	25,112	25,112	25,112

Standard errors in parentheses

Standard errors clustered by country.

Tenure omitted in models (3) and (6) due to collinearity with Survey FE.

⁺ $p < 0.10$, * $p < 0.05$

S10: Mismatch in Tenure

In table S.8.12, we re-estimate the models from table 8.1, including a control for whether the tenure of the executive officer (i.e. president or prime minister) is different from that of the executive party. We include this control by itself and interact it with time in office. This control is, obviously, highly co-linear with time in office. As such, the longer a party is in office, the more likely it is that it switches out the executive officer. Adding these controls shift the estimates and standard errors slightly, making the interaction estimate significant at the .1 level rather than at the 0.05 level in columns one and four.

Table S.8.12 Linear regression of changes in executive party vote share

	(1)	(2)	(3)	(4)
Mismatch tenure (person v. party)	-3.13 ⁺ (1.85)	-3.17 ⁺ (1.88)	-2.78 (2.15)	-3.45 (2.29)
Economic growth	0.68* (0.24)	0.76* (0.27)	0.59* (0.28)	0.88* (0.30)
Tenure	-0.11 (0.24)	-0.08 (0.23)	-0.15 (0.21)	-0.31 (0.28)
Mismatch tenure (person v. party) × Tenure	0.22 (0.25)	0.21 (0.25)	0.02 (0.29)	0.27 (0.29)
Economic growth × Tenure	-0.05 ⁺ (0.03)	-0.07* (0.03)	-0.04 (0.03)	-0.05 ⁺ (0.03)
Executive election	-2.80 ⁺ (1.49)	-1.82 (1.50)	-1.01 (1.95)	-3.90* (1.50)
Year FE		✓	✓	
Country FE			✓	✓
Leader FE				✓
Observations	409	409	409	409

Standard errors in parentheses

Standard errors clustered by country.

⁺ $p < 0.10$, * $p < 0.05$

S11: Two-step Models of Individual-level Data

Another way to examine whether there is an interaction between time in office and national economic perceptions is to estimate a multilevel model which allows for a random slope with respect to national economic perceptions across the different surveys, and then examine whether the size of the survey-specific slopes are related to the tenure of the incumbent party at the time of the survey.

To do this, we estimate a set of multi-level logit models of the probability of voting for the executive party with the full set of individual-level controls, omitting time in office, allowing the effect of national economic perceptions to vary across the surveys (i.e., estimate a random slope model). We estimate three of these models: one with survey random effects, one with leader fixed effects and one with survey fixed effects. For each of these models, we obtain sixty different logit coefficients, which represent the effect of national economic perception in the individual surveys. We plot these logit coefficients against incumbent tenure at the time of the survey for each of the three models in figure S.8.3.

As can be seen from these figures, there is a negative relationship between time in office and the size of the logit coefficients. OLS regressions of time in office on the logit coefficients reveal that the negative relationship is statistically significant ($p < 0.05$, using country-clustered standard errors). As such, this alternative way

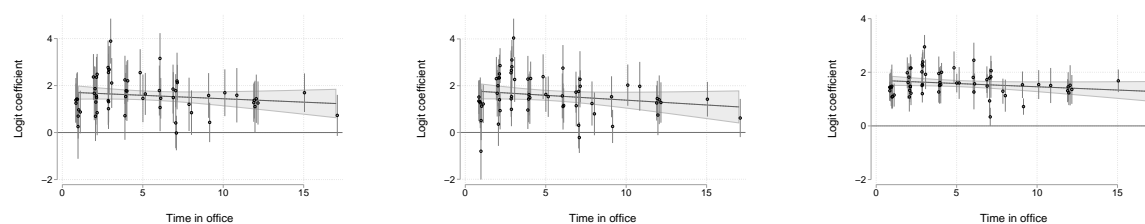


Figure S.8.3 Random slope of NEP plotted with 95 pct. confidence intervals. From left to right the models used for plotting include random effects, leader fixed effects and survey fixed effects. Uniformly distributed random noise added to the horizontal placement of the dots. The figure shows a linear fit with 95 pct. confidence intervals.

of estimating the effect of time in office on the economic vote gives the same basic result as that identified in table 8.2.

S12: The Role of Prospective Economic Conditions

In table S.8.13, we re-estimate the models presented in table 8.2, adding a measure of prospective national economic perceptions and an interaction between these perceptions and tenure.

To measure prospective national economic perceptions, we use the following question from the EES: “Over the next 12 months, how do you think the general economic situation in this country will be: a lot better, a little better, stay the same, a little worse, or a lot worse?”. We rescale this variable to go from zero (a lot worse) to one (a lot better). This question was not asked in the ’89 and the ’99 EES, and we therefore omit these years when estimating the models with prospective economic perceptions.

As can be seen from table S.8.13, there is no statistically significant interaction between prospective economic perceptions and time in office. Further, the negative interaction between time in office and (retrospective) national economic perceptions remains statistically significant, and is three times the absolute size of the interaction for prospective economic conditions. In sum, there is no evidence that incumbents time in office lead voters to shift their focus from one type of economic perceptions to another. They simply become less reliant on retrospective national economic conditions when deciding whether to vote for the incumbent.

Table S.8.13 Multi-level logit model of voting for executive party

	(1)	(2)	(3)
Prospective NEP	0.27 (0.18)	0.28 (0.18)	0.27 (0.18)
National Economic Perceptions	1.67* (0.27)	1.61* (0.27)	1.71* (0.28)
Tenure	0.06 (0.04)	0.05 (0.05)	
Prospective NEP × Tenure	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)
National Economic Perceptions × Tenure	-0.06* (0.03)	-0.06* (0.03)	-0.07* (0.03)
Lagged executive party vote	4.28* (0.17)	4.29* (0.16)	4.29* (0.17)
Ideology	2.43* (0.33)	2.44* (0.33)	2.44* (0.34)
Religiosity	0.09 (0.15)	0.09 (0.15)	0.09 (0.15)
Class	0.29* (0.15)	0.30* (0.15)	0.28 ⁺ (0.15)
Survey RE	✓	✓	✓
Leader FE		✓	✓
Survey FE			✓
Observations	28,894	28,894	28,894

Standard errors in parentheses

Standard errors clustered by country.

Tenure omitted in model (3) due to collinearity with Survey FE.

⁺ $p < 0.10$, * $p < 0.05$

S13: Endogeneity in National Economic Perceptions

In the individual-level data, we generally found higher levels of economic voting across all levels of tenure than we did in the country-level data. As mentioned above, this might be because the endogeneity of national economic perceptions is leading us to overestimate the level of economic voting in the individual-level data. To investigate whether this is the case, we re-analyze the individual-level data in two different ways, both of which might allow us to sieve out some of this endogeneity.

First, we examine a smaller, restricted sample. Specifically, we exclude those who voted for the incumbent at the last election, because these are more likely to be incumbent partisans and thus engaged in the type of “wishful thinking” described above.¹⁰ In the first three columns of table S.8.14, we present the results of this analysis for the same model specifications that were used in the main analysis. Figure S.8.4 plots the average marginal effects of national economic percepts across tenure for the censored sample, based on the model presented in column three of table S.8.14. As can be seen from this figure, the pattern identified in this censored sample matches up more closely with that found for the country-level data. As such, when we leave out incumbent “partisans”, incumbent tenure tends to completely crowd out economic voting.

Second, we use aggregate objective economic conditions to instrument national economic conditions. This approach sidesteps problems with endogeneity by only examining the differences in national economic perceptions which are caused by changes in objective economic conditions (see Nadeau, Lewis-Beck and Bélanger, 2013, for the details of this method). This means sieving out variation in national economic perceptions which are caused by other factors, such as partisanship.

In measuring objective economic conditions we include election year inflation, unemployment and economic growth at the country-level. All these variables were taken from the World Banks database. The reason we do not simply use economic growth, as we did in the analysis of the country-level data, is that it is possible to get estimates for unemployment and inflation for the time period covered by the EES. The same was not possible for the country-level data. Turning to estimation, we instrument national economic perceptions and the interaction between these percepts and tenure using growth, unemployment and inflation as well as an interaction between these three variables and time in office. We omit the individual-level controls, since these are potentially endogenous as well, and we do not use survey or leader

¹⁰A more standard measure of party identification would be preferable, however, no such measure is included in the EES.

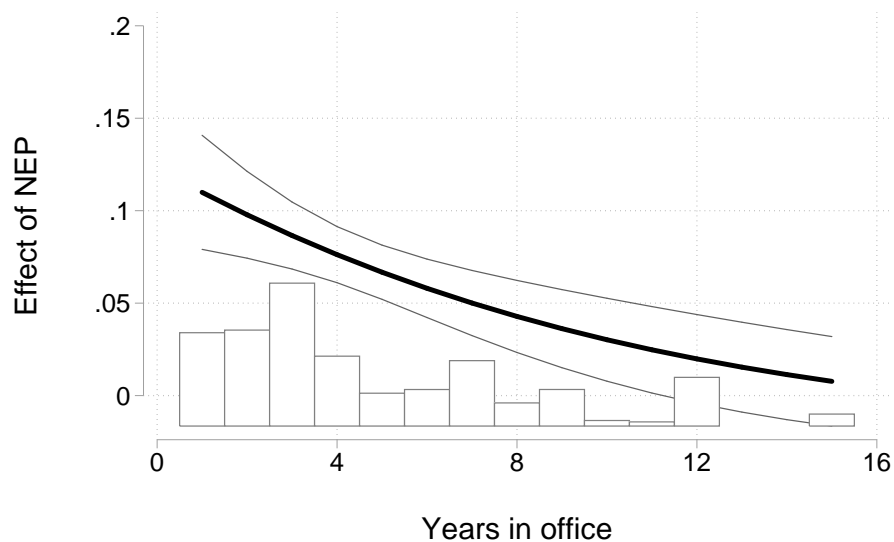


Figure S.8.4 The average marginal effects of national economic perceptions on the probability of voting for the executive party across levels of tenure with 90 pct. confidence intervals. Estimated based on the model presented in column three of table S.8.14. Derived from the model using respondents who did not vote for the executive party at the last election with survey fixed effects. The bar plot shows the density of the variable years in office.

Table S.8.14 Addressing Endogeneity Problems

	(1)	(2)	(3)	(4)
National Economic Perceptions	2.41*	2.53*	2.51*	0.39*
	(0.31)	(0.31)	(0.31)	(0.04)
Tenure	0.05	0.00		0.01*
	(0.03)	(0.04)		(0.00)
National Economic Perceptions × Tenure	-0.13*	-0.18*	-0.14*	-0.02*
	(0.05)	(0.05)	(0.05)	(0.01)
Ideology	2.98*	3.01*	2.99*	
	(0.28)	(0.28)	(0.28)	
Religiosity	-0.13	-0.15	-0.12	
	(0.14)	(0.15)	(0.14)	
Class	0.27	0.30 ⁺	0.29	
	(0.17)	(0.17)	(0.18)	
Survey RE	✓	✓	✓	
Leader FE		✓	✓	
Survey FE			✓	
R ²				
RMSE				0.45
Observations	25,220	25,220	25,220	47,967

Standard errors in parentheses

Standard errors clustered by country.

Tenure omitted in model (3) due to collinearity with Survey FE.

⁺ $p < 0.10$, * $p < 0.05$

fixed effects as these would be perfect or near-perfectly collinear with the aggregate level economic indicators. Finally, we link the instrumented economic perceptions and incumbent support using a linear probability model rather than a logit model to make the estimation less computationally complex.

The estimates produced using this instrumental variables approach are presented in the fourth column of table S.8.14. As can be seen from this model, we still see a statistically significant negative interaction between tenure and the now-instrumented national economic perceptions. In figure S.8.5, we plot the average marginal effects based on the instrumental variables regression. Here, we see that after taking potential problems with endogeneity into account, the level of economic voting becomes statistically indistinguishable from zero after roughly 15 years in office. This trajectory is roughly similar to what we find in the country-level data, where the effect of economic voting also becomes statistically indistinguishable from zero as time in office increases (although this already happens after eight years, cf. figure 8.1).

Taken together, these analyses lend credibility to the conclusion that both the individual-level and country-level dataset paints a similar picture of the relationship between time in office and economic voting.

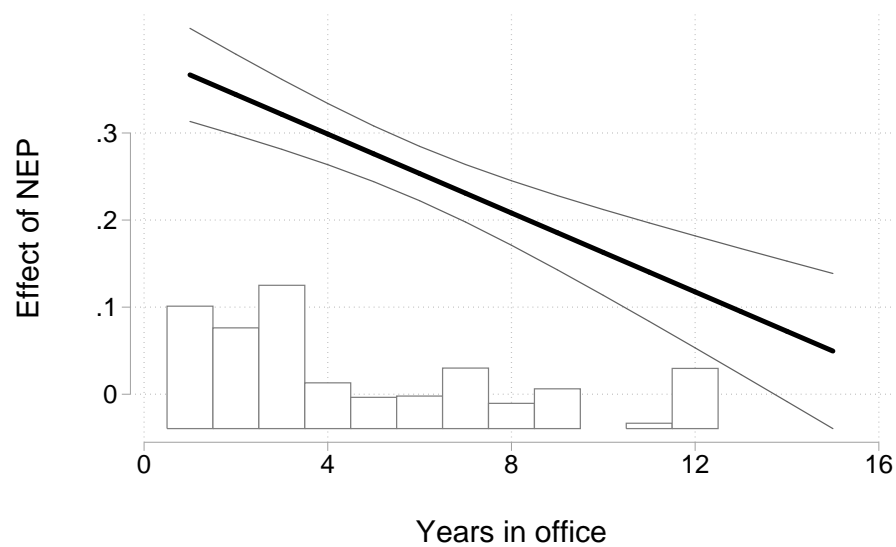


Figure S.8.5 The average marginal effects of national economic perceptions on the probability of voting for the executive party across levels of tenure with 90 pct. confidence intervals. Derived from the instrumental variables estimation, cf. column four of table S.8.14. The bar plot shows the density of the variable years in office.

Chapter 9

A Self-serving Bias in Attribution of Political Responsibility

Individuals' desire to protect and enhance their own self-image often lead them to take personal responsibility for good outcomes yet externalize responsibility for adverse outcomes. In this article, I show that this self-serving bias in attribution has important implications for how voters assign political responsibility in settings where it is unclear whether outcomes are a result of government intervention or an individual's own behavior. In these settings, voters assign political responsibility in a self-serving way, downplaying the government's role in producing desirable outcomes, and highlighting the government's role in producing undesirable outcomes. I demonstrate that voters attribute responsibility in this way using election studies from three different continents, a survey asking detailed questions about attributions, and a set of survey experiments.

The protection and enhancement of one's self-image is an important motivation underlying human behavior (Baumeister, 1999; Beauregard and Dunning, 1998; Sedikides and Strube, 1995). This is reflected in a ubiquitous self-serving bias in attribution, which motivates people to draw causal inferences that make themselves look good (Heider, 2013; Kunda, 1999; Stephan, Rosenfield and Stephan, 1976). In particular, because of this bias, people tend to take personal responsibility for desirable outcomes and externalize responsibility for undesirable outcomes. Arguably, this self-serving bias is also present when people make attributions about the outcomes of political decisions, and it might therefore, in some cases, affect how voters attribute political responsibility. In spite of this, no study has examined whether or under what conditions the self-serving bias affect voters' political attributions.

While the previous literature has not examined whether voters' desire to make *themselves* look good affects how they attribute responsibility for policy outcomes, a large number of studies have found that voters' desire to make their *preferred party* look good does make a difference. As such, voters who identify with, or feel close to, a particular party will hold this party responsible for desirable policy outcomes yet exculpate the party for undesirable outcomes (Bisgaard, 2015; Healy, Kuo and Malhotra, 2014; Malhotra and Kuo, 2008; Marsh and Tilley, 2010; Rudolph, 2003*b*, 2006; Tilley and Hobolt, 2011). This fits nicely with the larger literature on how attachment to a party fundamentally alters voters' political cognition (e.g., Campbell et al., 1960). A partisan bias in attribution is also consistent with a self-enhancement motive, as partisans might selectively attribute in order to protect the image of themselves as someone who supports a successful party (for an argument along these lines, see Tilley and Hobolt, 2011, 319). However, the effects of voters' self-serving motives on their political attributions might extend beyond any role these play in driving the partisan bias.

This article argues that self-serving motives shape how voters assign political responsibility in settings where it is unclear whether outcomes are the result of government intervention or an individual's own behavior. This includes a number of important policy outcomes such as voters' employment situations, their mortgage payments, their children's test scores, and the quality of their health insurance. When it comes to this type of policy outcome, concerns about self-image should motivate voters to shift responsibility for undesirable outcomes away from themselves and towards governing politicians, and similarly motivate voters to shift the responsibility for desirable outcomes away from politicians and towards themselves. For instance, we would expect voters to be more likely to attribute responsibility to the government

for losing a job as opposed to getting a job, because individual voters can influence their own employment prospects, and therefore should want to downplay (overstate) their own responsibility for adverse (good) employment outcomes. Conversely, when it is clear that outcomes are not the result of an individual's own behavior, the self-serving bias should not play any direct role in shaping voters' attribution of responsibility for these outcomes. For instance, voters should not be more likely to attribute responsibility to the government for increases as opposed to decreases in the national unemployment rate, because individual voters cannot affect the national unemployment rate, leaving them with no self-serving motive to attribute political responsibility asymmetrically.

In order to empirically substantiate this argument, I undertake three separate studies. The first study examines voters' propensity to support the incumbent across their beliefs about the national and their personal economic situation. Using data from the Danish National Election Studies, the American National Election Studies and the Latinobarómetro, I find that voters electorally punish incumbents when they think their personal economic situation is deteriorating, yet do not reward the government when they think their personal economic situation is improving. I find no similar asymmetry across evaluations of the national economic situation. This is consistent with the self-serving bias in attribution, because voters can influence how their personal economic situation develops, and therefore have a self-serving motive to attribute less (more) responsibility to the government when their economic situation is improving (deteriorating). They have no similar motive when it comes to the national economic situation.

In the second study, I move a step closer to the proposed mechanism, investigating whether voters adjust their beliefs about the extent to which the government is implicated in producing economic outcomes in a self-serving way. To that end, I use a population-based survey of Danish voters, asking respondents about whether they believe the government can affect their personal economic situation, and look at how these beliefs correlate with voters' evaluation of their personal and the national economic situation. In line with what I would expect if there is a self-serving bias in political attribution, I find that voters who believe their own economic situation is doing better are less likely to believe that the government can affect their own economic situation.

In the third study, I address issues related to causal inference: issues which are hard to get at with the observational data used in studies 1 and 2. To do this, I conduct a survey experiment on a population-based sample of Danish voters, as

well as a survey experiment on a convenience sample. In the experiments, I ask respondents to evaluate the extent to which the government would be responsible for a set of hypothetical outcomes, randomly assigning outcomes to respondents. Across different types of outcomes, I find that when it comes to policy outcomes voters' might have had a hand in shaping, they hold the government more responsible for undesirable outcomes than for desirable outcomes.

The article extends the literature on how voters assign political responsibility for policy outcomes, as it tells us that not only partisanship but also self-servingness can bias voters' attributions. Further, the article uses both observational and experimental data from outside the US and the UK in a literature which has been primarily experimental and based on these two countries (Healy, Kuo and Malhotra, 2014; Malhotra and Krosnick, 2007; Marsh and Tilley, 2010; Rudolph, 2006; Tilley and Hobolt, 2011). The findings may also help explain why previous literature has generally found the effects of personal economic conditions to be small and inconsistent (Kinder and Kiewiet, 1979, 1981; Lewis-Beck and Stegmaier, 2013; Singer and Carlin, 2013; Stubager et al., 2014). As such, if the self-serving bias drives down the effect of personal economic conditions when these conditions are improving, then the estimated effect of these conditions will be sensitive to the distribution of personal economic conditions in the electorate (i.e., be smaller when more people's personal economic situation is improving).

To the extent that the way voters interpret and act on policy outcomes can shape the actions of reelection-minded politicians (Alt, De Mesquita and Rose, 2011; Besley, 2007; Ferejohn, 1986; Tilley, Neundorf and Hobolt, 2017), the self-serving bias also has interesting implications for democratic accountability. In particular, politicians might shy away from otherwise efficient policies, if these policies leave room for interpretation as to whether their outcomes are due to the policy or due to the actions of the individual voter, because voters will seize upon this ambiguity, and assign responsibility for any gains to themselves and responsibility for any losses to the politicians.

The Political Relevance of the Self-serving Bias

Social psychological research has long identified a self-serving bias in attribution (Fiske and Taylor, 2013; Greenwald, 1980; Heider, 2013; Stephan, Rosenfield and Stephan, 1976, 272). This bias is reflected in a "tendency for people to take personal responsibility for their desirable outcomes yet externalize responsibility for their

undesirable outcomes" (Shepperd, Malone and Sweeny, 2008, 895). If someone, for instance, gets a good grade on an exam, they will infer that this must be based on their own effort and skill, however; if they get a bad grade, they infer that it was due to the teacher's tough grading or the loud neighbors who made studying impossible (McAllister, 1996).

The self-serving bias is a type of "directional" motivated reasoning (Kunda, 1990), meaning that the bias leads people to reach conclusions based on some other motive than accuracy. In particular, the self-serving bias has been shown to be driven by a number of different cognitive heuristics and psychological needs (Shepperd, Malone and Sweeny, 2008; Snyder, Stephan and Rosenfield, 1976). The most prominent driver of the bias, however, is the need to sustain a positive self-image (i.e., self-enhancement or self-protection) (Miller, 1976; Sedikides, Gaertner and Toguchi, 2003).

While previous studies have found that the self-serving bias shapes people's attributions in a number of different areas (e.g., Campbell et al., 2000), it has not been shown to affect how voters attribute responsibility for policy outcomes. This might be because the self-serving bias has no obvious universal relevance for this type of outcome. To see this, imagine that a voter thinks the national economy is doing worse than it was 12 months ago. It is not obvious how the self-serving bias should influence the attribution of responsibility for this economic development. However, even if the the self-serving bias does not have an impact on how voters attribute responsibility for all types of policy outcomes, it might still have an impact in some cases. In particular, the self-serving bias might play a role when the policy outcome voters attribute responsibility for satisfies the following *plausibility criteria*.

Voters need to believe that they can *plausibly* influence the outcome themselves. Since the self-serving bias is premised on voters wanting to adjust how personally responsible they are for an outcome based on how desirable this outcome is, the bias is obviously not relevant if voters know, on the face of it, that they had no personal responsibility for said outcome. Imagine, for instance, that a voter want to assign responsibility for rising inflation. Since the voter knows that she had nothing to do with increasing prices, adjusting the extent to which she is responsible *vis-a-vis* the government does not make sense. Imagine instead that a voter's mortgage payments go up. The voter knows that he is partly responsible for the size of his mortgage payments, as he could (plausibly) have negotiated a better deal with the bank or increased his savings earlier in life. Accordingly, the voter can meaningfully adjust the extent to which he is responsible *vis-a-vis* the government, making the self-serving

bias relevant. The first criteria, therefore, is that voters need to be plausibly personally responsible for the outcome.

In addition to this, voters need to believe that political control over the outcome is ambiguous. As such, if voters believe the economic outcome is either completely under political control or completely out of political control, voters will not be able to rationalize adjusting the extent to which the government is responsible. The second criteria, therefore, is that it is plausible for voters to adjust how responsible the government is for the outcome.

These plausibility criteria matter because even when people are attributing selectively they “do not seem to be at liberty to conclude whatever they want [...] [t]hey draw the desired conclusion only if they can muster of the evidence necessary to support it.” (Kunda, 1990, 482-283). Put differently, if voters are to shift responsibility for an outcome self-servingly between themselves and the government, they need to be able to think up a set of reasons why they themselves (criteria one) and the government (criteria two) had some part to play in producing this outcome. If the plausibility criteria are not met, this is impossible.

If both plausibility criteria are met, however, the self-serving bias should shape voters’ attributions of political responsibility. In an attempt to minimize their own involvement, voters should over-emphasize the role played by the government in producing desirable outcomes, and, in an attempt to maximize their own involvement, voters should under-emphasize the role played by the government in producing undesirable outcomes. That is, if the plausibility criteria are met, there will be a valence asymmetry in the extent to which voters attribute responsibility to the government. This is the article’s central hypothesis:

The Self-serving Bias Hypothesis: If voters can reasonably assign responsibility to themselves and to governing politicians for a given outcome, then voters will hold governing politicians more responsible for this outcome if it is undesirable as opposed to desirable.

The Hypothesis and the Existing Literature

The hypothesis is most closely related to a small and recent set of studies which explain policy attitudes in terms of the self-serving bias (e.g., Cassar and Klein, 2017; Deffains, Espinosa and Thöni, 2016; Joslyn and Haider-Markel, 2017). However, unlike these studies, this hypothesis is not concerned with voters’ preferences for par-

ticular policies, but in how voters attribute political responsibility to the government for policy outcomes.

As mentioned in the introduction, the hypothesis is also related to the large literature on partisan bias in political attributions (e.g., Bisgaard, 2015; Healy, Kuo and Malhotra, 2014; Malhotra and Kuo, 2008). In fact, the partisan bias in attribution is often conceptualized as the “group-serving” counterpart to the self-serving bias (Rudolph, 2003*b*, 701), and might be driven by similar psychological impulses (Lodge and Taber, 2013; Tilley and Hobolt, 2011). Even so, the self-serving bias hypothesis stands apart from the partisan attribution bias hypothesis in that it is interested in whether voters exculpate themselves, rather than their preferred party, when assigning blame for undesirable outcomes.

As the self-serving bias hypothesis predicts a valence asymmetry in attribution of responsibility for policy outcomes, it is also related to studies which identify a negativity bias (or grievance asymmetry) in retrospective voting (cf. Bloom and Price, 1975; Soroka, 2006). However, the self-serving bias hypothesis suggests that the valence asymmetry is conditional upon the type of outcome (e.g., conditional on whether voters can influence the outcome themselves), contrary to the negativity bias literature, where the asymmetry is typically thought to be unconditional (cf. Nannestad and Paldam, 1997). Even so, the hypothesis is not necessarily at odds with the notion of a more unconditional negativity bias, as the present hypothesis focuses exclusively on attribution and not on how voters form beliefs about the quality of policy outcomes. Accordingly, a conditional attributional valence asymmetry could be compatible with an unconditional perceptual valence asymmetry. In particular, the self-serving bias hypothesis is consistent with a world in which voters are always more (less) likely to notice, remember and retrieve negative (positive) information, when assessing the quality of policy outcomes. The self-serving bias hypothesis simply suggests that once the voter has assessed the quality of a given policy outcome, then the valence of this assessment will only affect the extent to which the voter attributes responsibility to the government if this outcome meets the two plausibility criteria laid out above.

The self-serving bias hypothesis is also indirectly linked to the literature on the relative importance of personal contra national economic conditions (Kinder and Kiewiet, 1979, 1981; Singer and Carlin, 2013; Stubager et al., 2014; Tilley, Neundorff and Hobolt, 2017). This becomes clear if one thinks systematically about the kinds of outcomes which voters can, and the kind of outcomes voters cannot, hold themselves responsible for. As such, voters can attribute responsibility to themselves for the

quality of their personal economic situation, but they cannot attribute responsibility to themselves for national economic conditions. When it comes to testing the hypothesis below I use this fact repeatedly. Even so, these empirical explorations will differ from previous research in this area because the focus will not be on the absolute or relative weight that voters put on national and personal economic conditions. Instead, the goal will be to get at whether there is a valence asymmetry in attribution of political responsibility, and whether this valence asymmetry is only present for personal economic conditions.

Testing the Hypothesis

As mentioned in the introduction, the article employs a series of surveys and experiments to test the self-serving bias hypothesis. I have organized these different empirical explorations into three separate studies. All three studies, directly or indirectly, look at how voters attribute responsibility for outcomes which meet and outcomes which do not meet the plausibility criteria. The three studies have different inferential strengths (i.e., some have stronger external and some have stronger internal validity), and thus serve as a type of methodological triangulation. Table 9.1 presents an overview of the different studies.

Table 9.1 Overview of the studies

Motivation	Data sources	N
1. Identify signs of a self-serving bias in voter behavior.	Danish National Election Survey (1990-2015)	13,292
	American National Election Survey (1984-2012)	13,306
	Latinobarómetro (1995-2010)	141,191
2. Explore whether differences in behavior reflect differences in attributions.	Survey of Danish voters (2014)	933
3. Explore whether differences in attributions are caused by the valence of policy outcomes.	Survey experiment with Danish voters (2015)	1,002
	Survey experiment with convenience sample (2017)	263

Study 1: Election surveys

Following the self-serving bias hypothesis developed above, voters should hold governing politicians more electorally responsible for worsening economic conditions than for improving economic conditions when it comes to their own economy. I expect there to be such a valence asymmetry because voters will be motivated to hold themselves less responsible when their economic conditions are deteriorating, leaving the door open for blaming the government instead. However, there should be no such valence asymmetry when it comes to national economic outcomes, as a bad (good) national economic situation does not reflect poorly (well) on the individual voter. The article begins to test the self-serving bias hypothesis by looking at whether voters attribute responsibility in this way in a large set of election surveys.

Election surveys generally do not include explicit questions about attribution. To measure how responsible voters hold their government for economic outcomes in these election surveys, I therefore assume that attribution of responsibility can be inferred from the correlation between voters' perceptions of the economy and support for the incumbent government. This is a relatively standard assumption in retrospective voting research (e.g., Carlin and Singh, 2015; Duch and Stevenson, 2008; Lewis-Beck and Stegmaier, 2013).

I use three different sets of election surveys: the American National Election Studies (ANES), the Latinobarómetro and the Danish National Election Studies (DNES). The ANES was chosen because much groundbreaking research on retrospective voting has used this dataset (e.g. Fiorina, 1981). The Latinobarómetro was chosen based on two considerations; (1) it covers a diverse set of countries that are quite different from the US, enhancing external validity; (2) it includes a large number of respondents ($n > 140.000$), increasing statistical power. Finally, studies 2 and 3 are based on surveys of Danish voters, so to increase consistency and continuity across the different studies, the DNES is included as well.

Data and Empirical Strategy

From the ANES, I use the time series cumulative data file, analyzing data from the presidential election years 1984 to 2012. From the Latinobarómetro, I use 141 annual surveys from 18 countries covering the years from 1995 to 2010 (our starting point is the dataset used in Carlin and Singh, 2015). The DNES data is from the Danish Data Archive, and covers all parliamentary elections from 1990 to 2015 (except 1998 due to a missing question concerning personal economic conditions). For an overview

of which surveys were included in study 1, see section S2 of the supplementary materials.

The dependent variable is support for the incumbent executive. In the ANES, this is operationalized as a dummy variable indicating whether the respondent reported voting for the incumbent presidential party at the presidential election. Denmark has a parliamentary system, and accordingly the dependent variable in the DNES is a dummy variable indicating whether the respondent reported voting for one of the parliamentary parties which were in government at the time of the election, and zero otherwise. Since the Latinobarómetro data does not follow election cycles, I cannot use reported voting behavior at elections as the dependent variable. Instead, I use a dummy variable indicating whether the respondent approved of the incumbent president's performance.¹

The independent variables are voters' evaluations of their own and the national economy. Specifically, I use two questions which were included in all the election surveys, which asks respondents how (1) your own and your family's economy and (2) the national economy has developed over a period of time. The time period differs across the different election surveys covering anything from the last 12 months to the last three years. Responses given to these questions were sorted into three categories: responses indicating the economy had stayed the same, responses indicating the economy had gotten worse, and responses indicating the economy had gotten better.²

I also use a small set of control variables in some parts of the analysis. These are gender, age in years, education, ideology and strength of party identification. Education is measured using a dummy indicating whether the respondent reported having attended college/university. Ideology is measured on an eleven-point scale going from left to right in the Latinobarómetro and the DNES. In the ANES, ideology is measured on a seven-point scale going from "extremely liberal" to "extremely conservative". Strength of party identification is measured on a three point scale in the DNES (none, weak, strong) and on a four point scale in the ANES (independent, leaning, weak and strong). The Latinobarómetro only asks for party identification in a small number of surveys. Instead, I use a dummy indicating whether the respondent had strong feelings about (not) trusting the president.

¹This is the standard dependent variable used when estimating retrospective voting models on the Latinobarómetro data (Carlin and Singh, 2015).

²For some of the election surveys, the respondent had more than three options available when characterizing the economic situation; however, to make comparisons across all of the surveys possible, these were collapsed into these three categories. Section S3 of the supplementary materials discusses whether this might affect the results.

To analyze the data, I model the probability of supporting the incumbent as a linear function of voters' evaluations of the national and their personal economic conditions. I also include survey fixed-effects to control for any election-specific or country-level confounders. For both national and personal economic evaluations, I include the variables as dummy variables, using those who thought their own/the country's economy had stayed the same as the reference category. I estimate the parameters of this linear probability model (LPM) using an OLS regression with robust standard errors, and estimate separate models for each of the three sets of election studies.

Results

Three of the four graphs in figure 9.1 present the key estimates from the OLS regressions run on the DNES, the ANES and the Latinobarometro data.³ In particular, it presents the estimated effect of evaluating the economy as doing *better* rather than having stayed the same, the estimated effect of evaluating the economy as doing *worse* rather than having stayed the same, and the estimated asymmetry in these effects: the valence asymmetry.⁴ The valence asymmetry represents the extent to which voters react more strongly when the economic situation changes for the worse rather than for the better. A positive valence asymmetry thus means that voters are more sensitive to things getting worse. I present estimates of the valence asymmetry for both the national economic situation and for voters' personal economic situation.

The key take-away from figure 9.1 is that in all of the DNES, the ANES and in the Latinobarómetro results, there is a statistically significant valence asymmetry for personal economic conditions ($p < 0.05$), yet no discernible valence asymmetry for national economic conditions. Further, if one compares the estimated valence asymmetry for personal and national economic conditions, one finds that the former is significantly larger than the latter in each of the three sets of election surveys ($p < 0.05$). This is in line with the self-serving bias hypothesis, which predicted a valence asymmetry in attribution of responsibility for personal economic conditions, where voters can plausibly adjust how responsible they view the government *vis-a-vis* themselves, but no asymmetry for national economic conditions, where it makes no sense for voters to make any such adjustments.

³Section S4 of the supplementary materials present the tables underlying these and subsequent figures in the article. See section S6 for descriptive statics on all variables used in the analysis.

⁴See section S1 of the supplementary materials for details on how the valence asymmetry was estimated.

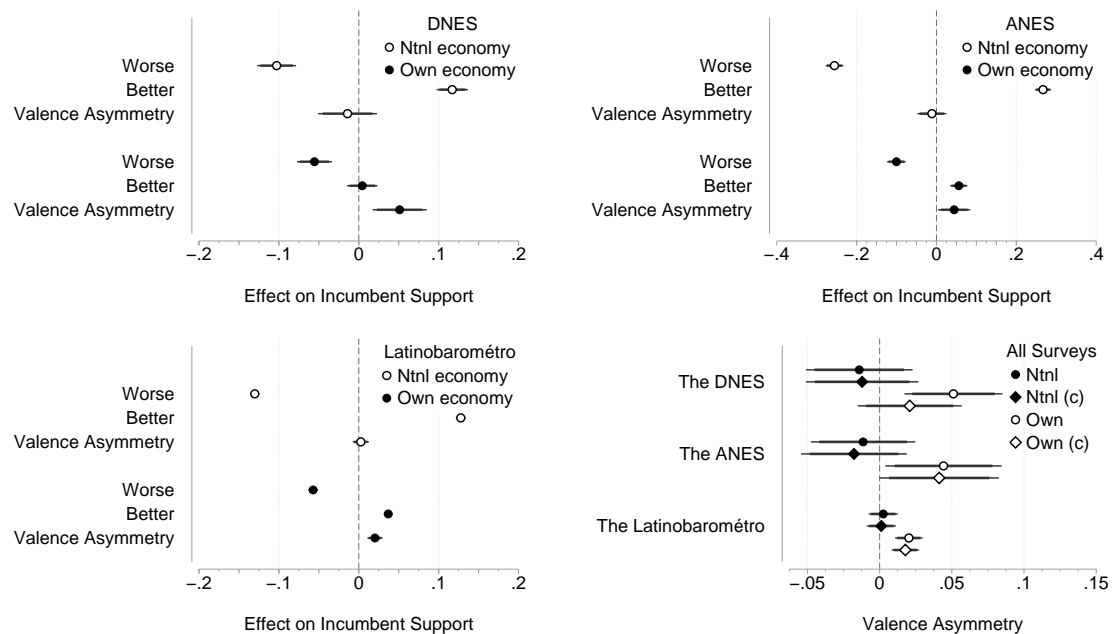


Figure 9.1 Estimated effects of believing your own or the national economy has gotten "Worse" or "Better" rather than "Stayed the same" on voting for/supporting the incumbent government. Estimated using OLS-regression of incumbent support for the DNES ($n = 13,293$), the ANES ($n = 13,306$) and the Latinobarómetro ($n = 143,191$). The valence asymmetry was calculated as the difference between the absolute values of the "Worse" and "Better" effects. Horizontal lines are 95 pct. (thin) and 90 pct. (thick) confidence intervals. Diamond shaped dots are from models which include controls (i.e., gender, age, education, ideology and strength of partisanship).

Figure 9.1 also shows, unsurprisingly, that across all three election surveys, the state of the national economy seems to be more closely related to government support than the state of the respondent's own economy (cf. Kinder and Kiewiet, 1981). There are also some differences across the election surveys. As such, for both the Latinobarómetro and the ANES, there is a statistically significant estimated effect of perceiving one's own economy as improving rather than staying the same. There is no such effect in the DNES. Even so, the valence asymmetry in the effect size is present in all three sets of election studies.

How does this pattern hold up to statistical control? To test this I re-estimate the models used to produce figure 1, including age, gender, education, ideology and strength of partisanship as controls. These variables are not meant to be exhaustive, as they do not control for all possible confounders. Nor are they necessarily great controls, as some of them, like ideology, might be post-treatment (King, 2010). However, in re-estimating the models using these controls, I can conduct a simple test of whether the patterns found above can be explained away by using a "standard set of controls".

In the graph presented in the bottom right corner of figure 9.1, I plot the valence asymmetry estimated from the LPMs with controls. For comparison, I also include similar estimates from the models without controls. The main difference is that the valence asymmetry for the respondent's own economy in the DNES becomes roughly two percentage points smaller and also statistically insignificant ($p = 0.26$). However, in the ANES and the Latinobarómetro, the valence asymmetry for the respondent's own economy is still significantly different from zero ($p < 0.05$) and significantly different from the valence asymmetry for the national economy ($p < 0.05$).

Just as important as the statistical significance, however, is that once controls are introduced, the approximate size of the valence asymmetry is remarkably similar across all three election studies (ca. two to four percentage points). This resonates with the idea that the self-serving bias in attribution is a relatively universal psychological mechanism.

In sum, voters in the ANES, the DNES and the Latinobarómetro behave in a way that is consistent with a self-serving bias in political attribution. Across the US, Latin America and in Denmark, voters are unlikely to hold incumbents responsible for their personal economy if it is improving, yet likely to hold incumbents responsible for their personal economy if it is getting worse. This is an asymmetry which is not present for the state of the national economy. These findings are especially noteworthy, because of the diverse set of contexts which have been analyzed. As

such, signs of a self-serving bias in political attribution do not seem confined to one particular type of election or country.

While the consistency of the results does speak in favor of the self-serving bias hypothesis, there are still some important inferential issues, which this data cannot deal with effectively. First, I assume that differences in the effect of economic conditions on incumbent support correspond to differences in attribution of responsibility. As mentioned above, this is a standard assumption in much research on retrospective voting, however, this does not necessarily make it a valid assumption. Second, I assume that the correlation between voters' beliefs about the economy and incumbent support reflects a causal effect of the former on the latter. This is not necessarily the case, as the factors which determine voters' beliefs about their own and the national economy might have an independent effect on incumbent support (i.e., there might be omitted variable bias). In studies 2 and 3, I devise new tests of the self-serving bias hypothesis, which try to address these shortcomings. In particular, study 2 utilizes a survey of voters' beliefs about the role government plays in producing economic outcomes. This should alleviate concerns related to the measurement of attribution of responsibility. Study 3 randomly assigns hypothetical outcomes to voters in the context of a survey experiment, which should help to alleviate concerns related to causal inference.

Study 2: A Survey of Voters' Attributional Beliefs

In study 2, I test the self-serving bias hypothesis by once again examining the relationship between voters' beliefs about the economic situation and the extent to which they attribute responsibility for this situation to the government. However, instead of gauging attribution of responsibility indirectly, I measure it directly by asking voters to what extent they believe the government can affect different aspects of the economy.

The expectations are similar to those in study 1. As such, I expect that voters who believe their own economic situation is improving will be less likely than those who think their own economic situation is deteriorating to think that the government can affect their personal economy. Conversely, I do not expect voters' evaluations of the national economy to have any bearing on voters' beliefs about the government's role in shaping national economic conditions.

Data and Empirical Strategy

The survey I use in study 2 was part of the “DK-OPT” project.⁵ The survey was collected by the polling company Epinion using a population-based sample frame. The survey ran from May 28 to June 28 2014, included 1,028 respondents, and was conducted over the phone. The sample was diverse though not completely representative of the Danish voting age population.⁶

The survey focused mainly on EU attitudes, but also included some items related to the national government's ability to affect the state of the economy. In particular, the survey included the following two items:

- “To what extent can the Danish government affect your personal economic situation?”
- “To what extent can the Danish government affect the national economic situation?”

Answers were recorded on five-point scales going from “Not at all” to “A lot”. These items are used as the dependent variable in the analyses. The independent variables are the same as in study 1; voters' evaluations of how their own economic situation and the national economy have developed over the last 12 months. For simplicity, responses are once again sorted into three categories; better, worse and the same.

I also use a small set of controls in this study; however, since strength of partisanship was not included in the survey, I only use ideology, education, age in years and gender. Once again, education is coded as a dummy indicating whether the respondent reported having attended university. Ideology is measured as agreement with the statement “People with high incomes should be more severely taxed”, measured on a five point scale from “Disagree Completely” to “Agree Completely”.

To analyze the data, I estimate two linear regressions with the two items on the government's role in shaping economic outcomes as the dependent variables. I include the independent economic variables as a set of dummy variables, using those who thought the economy had stayed the same as the reference category. I estimate both models with the small battery of controls and with robust standard errors.

⁵Data obtained from PI in the “DK-OPT” project, Derek Beach, Professor, Aarhus University, Denmark.

⁶The sample was more educated and slightly older than the Danish voting age population, cf. section S6 of the supplementary materials.

Results

Figure 9.2 presents the predicted values from these linear regressions. In the top panel, I look at how personal economic conditions are related to attributional beliefs. Here I find that voters are less likely to think the government can affect their personal economic situation if their own economic situation is improving. In particular, there is a statistically significant difference between those who think their economy is doing better than a year ago and those who think their economy is doing worse ($p < 0.05$). This is in line with a self-serving bias in attribution of political responsibility, since those who are doing better should be motivated to credit themselves rather than the government. Conversely, there is no similar relationship between voters' personal economic conditions and their tendency to believe the government is responsible for national economic conditions. This is important, because it tells us that the types of people who are doing well are not less likely to hold the government responsible for all types of economic outcomes. They are only less likely to hold the government responsible for their *own* good fortune, not the fortune of the nation as a whole.

In the bottom panel of figure 9.2, I look at how national economic conditions are related to attributional beliefs. In general, I find no systematic relationship between how voters believe the national economy is doing and their beliefs about the extent to which the government can affect their personal or national economic conditions. If I compare those who believe the country is doing better than a year ago with those who think the economy is doing worse, I find no differences in their attributional beliefs ($p > 0.2$ for both dependent variables).⁷

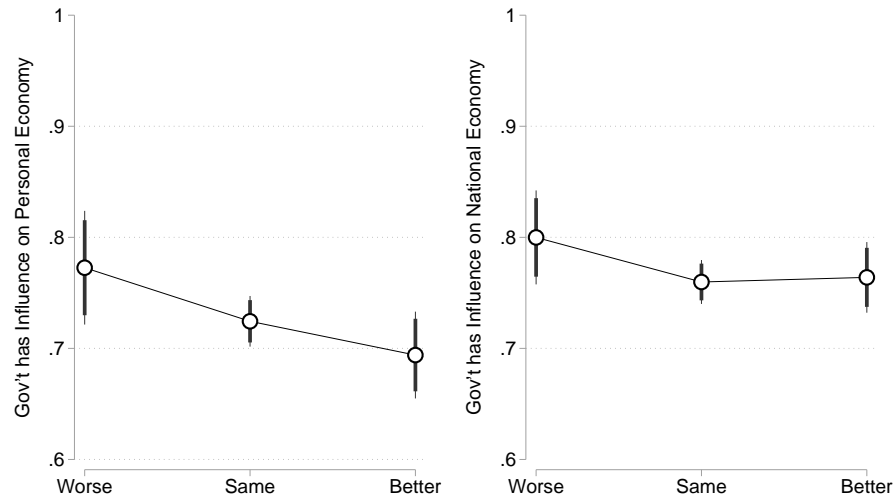
In sum, I find a relationship between the valence of personal economic conditions and the extent to which voters think the government can affect their personal economy, but no relationship between the valence of national economic conditions and the extent to which voters think the government can affect the national economy. This is exactly what the self-serving bias hypothesis would predict.

Study 3: Survey Experiments

In study 3, I test the self-serving bias hypothesis using a set of survey-experiments. In particular, I randomly assign voters to descriptions of different hypothetical outcomes, ask them the extent to which they believe the government would be responsible

⁷Interestingly, those who think the national economy has remained the same are significantly more likely to think that the government cannot influence their personal and the national economy.

Personal Economy



National Economy

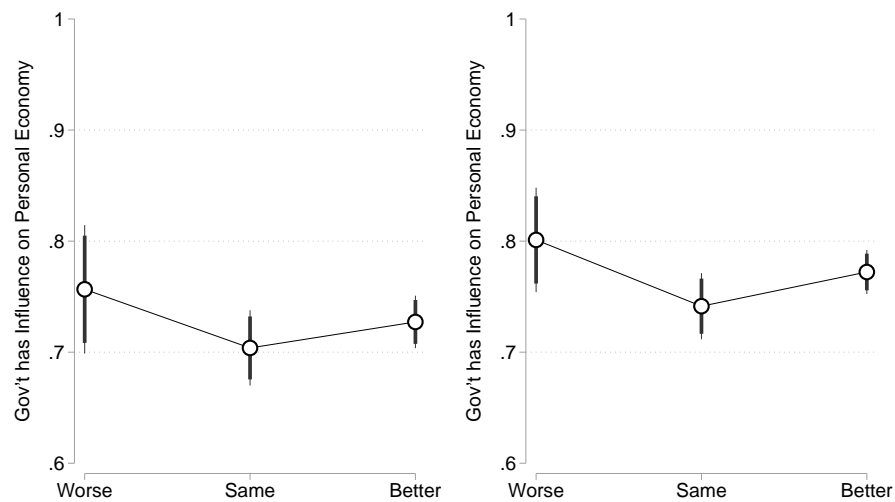


Figure 9.2 Top: Beliefs about the extent to which the government can affect your personal (left) and the national (right) personal economy across beliefs about personal economic conditions. Bottom: Beliefs about the extent to which the government can affect your personal (left) and the national (right) personal economy across beliefs about national economic conditions. Dots calculated by adding average marginal effects of economic variables, derived from the OLS-regressions ($n = 933$ for all models), to the sample mean. All models include both evaluations of your own and the national economy as well as controls for ideology, education, gender and age. Vertical lines are 95 pct. (thin) and 90 pct. (thick) confidence intervals.

for these outcomes, and then examine whether their answers follow the same self-serving pattern identified in studies 1 and 2.

By randomly assigning economic outcomes to voters, I address a key problem with the analyses I have engaged in so far, namely, that observed economic outcomes are endogenous to assignment of responsibility. So far I have estimated the effect of economic outcomes on the assignment of political responsibility by comparing voters who believe an outcome is getting better with voters who believe the same outcome is getting worse. This is potentially problematic, as voters with specific propensities to attribute responsibility to the government may, inadvertently or intentionally, select into specific types of beliefs.⁸ By assigning outcomes at random, we can be sure that voters' propensities to hold the government responsible is balanced, in expectation, across those assigned to desirable and undesirable outcomes.

In the first experiment, I vary two of the characteristics of the hypothetical outcomes that voters are assigned to. One is the valence of the outcome (i.e., whether it is positive, negative or neutral). The other is whether voters can reasonably assign responsibility to themselves (i.e., the first plausibility criteria). Following the observational studies, I manipulate this by presenting voters with outcomes at a personal or at a national level.

Data and Empirical Strategy

The survey experiment was conducted by the polling company Norstat using a population-based internet panel to recruit respondents. The survey ran from June 2 till June 4 2015. It sampled 1,002 respondents. The sample was diverse, though not completely representative of the Danish voting age population.⁹

The survey presented voters with two experimentally manipulated outcomes. One outcome was related to housing and one outcome was related to employment. For each of the two outcomes, respondents were given one of three valence conditions (negative, neutral, positive) and one of two relevance conditions (personal/national). Respondents were then asked: "To what extent would the government be responsible for this outcome?" Answers were given on a eleven point scale from "Not at all" to "A great deal". The variable was rescaled to go from zero to one.

⁸For instance, conservative voters may be more skeptical of the government's ability to affect the economy, refraining from attributing political responsibility for economic conditions, and at the same time, they may be more likely to observe good economic conditions, because conservatives tend to be more well off (Rudolph, 2003b, 2006).

⁹In particular, the sample was slightly older and had a higher proportion of men, cf. section S6 of the supplementary materials.

The first outcome voters were presented with concerned housing prices. Specifically, respondents were presented with one of the following six hypothetical outcomes:

1,2,3 Imagine that the price of your or your family's house [increased/ decreased/ increased or decreased].

4,5,6 Imagine that the price of houses in the country as a whole [increased/ decreased/ increased or decreased].

The positive economic outcome in this case is increasing house prices, which will enable voters to sell their house, or draw up a larger mortgage, at a possible gain to themselves (Ansell, 2014). Conversely, decreasing house prices is the negative outcome. Note that the neutral condition simply asks voters to evaluate how responsible the government would be for house prices either increasing or decreasing.

The second outcome concerned employment. Respondents were presented with one of the following six versions of the outcome:

1,2,3 Imagine that you or someone in your family [lost their job/ got a better job/ lost their job or got a better job].

4,5,6 Imagine that unemployment in the country as a whole [increased/ decreased/ increased or decreased].

For the first three versions, the positive outcome is getting a better job and the negative outcome is losing a job. For the last three versions, the negative outcome is increasing unemployment and the positive outcome is decreasing unemployment. The neutral outcomes are, once again, either the negative or the positive outcome.

The housing and employment outcomes have different inferential strengths and weaknesses. The balance across negative and positive outcomes is strong for house prices but weaker for employment status. As such, there might be different causal processes involved in losing a job contra getting a better job, whereas the causal processes involved in increasing contra decreasing house prices are more similar. At the same time, it might be hard for voters to figure out what implications increasing or decreasing house prices will have for their personal economic situation: that is, whether the housing outcome they get is in fact desirable or undesirable. Conversely, almost all voters should understand that getting a better job is a desirable outcome, and that losing a job is an undesirable outcome. All in all, the housing outcomes thus provide a harder test of the self-serving bias hypothesis and the employment

outcomes an easier test. As such, by including both outcomes the experiment should provide a fair overall test of the hypothesis.

I analyze the survey experiment by setting up two linear models, which use voters' attributions of responsibility for the housing and unemployment outcomes as the dependent variables. The independent variables are the different experimental treatments. The models are estimated using an OLS regression with robust standard errors.

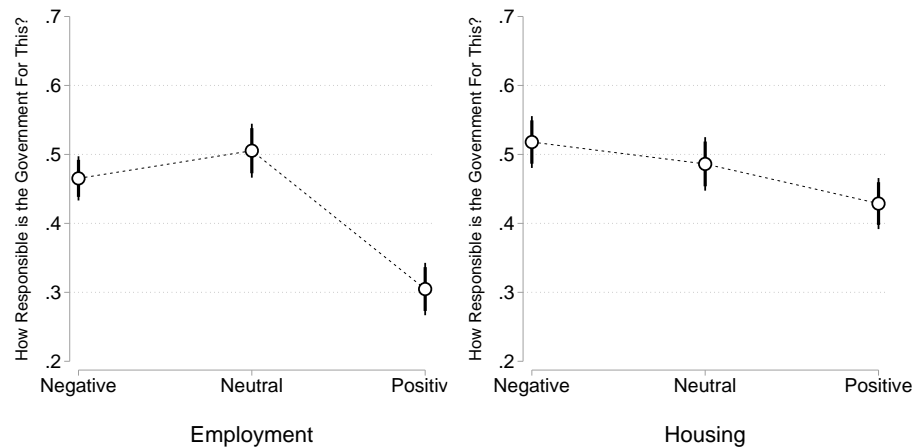
Results

Figure 9.3 presents the results from the main survey experiment. In the top panel, I examine the effects of the personal housing and employment outcomes. Across both types of outcomes, a similar pattern emerges. Voters who were assigned to a positive outcome were *less* likely to think that the government was responsible for securing this outcome than those assigned to a neutral or negative outcome. At the same time there was no statistically discernible difference between receiving a negative economic outcome as opposed to a neutral outcome. This conforms fairly well to the predictions made by the self-serving bias hypothesis. That is, when voters had a personal stake in the attributional process, they adjusted the extent to which they implicated the government in a self-serving way, downplaying the role of the government when faced with a desirable outcome.

In the bottom panel of the figure 9.3, I examine the effects of the national housing and employment outcomes. For the national housing outcomes there was practically no difference in the extent to which voters assigned responsibility to the government for positive, negative or neutral outcomes. For the national employment outcomes there was no difference in how voters assigned responsibility for positive or negative outcomes. Voters who were assigned a neutral national employment outcome, however, were less likely to assign responsibility to the government than those who were assigned to a positive or a negative outcome. The absence of systematic differences in attributions across negative and positive national level outcomes is also in line with the self-serving bias hypothesis, because there is no self-serving motive when it comes to assigning responsibility for national level outcomes that voters had no hand in shaping.

One finding from the experiment does not line up that nicely with the self-serving bias hypothesis: there is no difference in the extent to which voters hold the government responsible for neutral and negative personal economic outcomes. One explanation for this might be that the "neutral" condition, which asks voters to assign

Personal Economic Conditions



National Economic Conditions

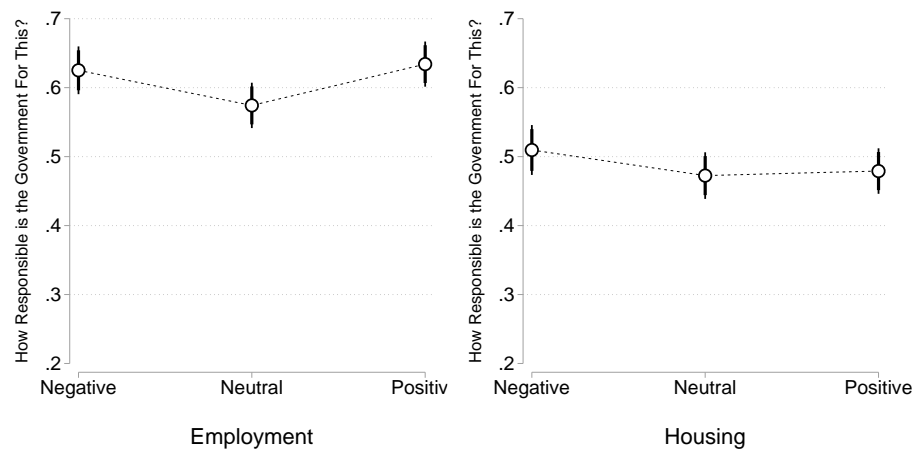


Figure 9.3 Mean level of beliefs about government responsibility for economic outcomes across valence ("Negative", "Neutral" and "Positive") and (personal and national). Mean levels reported separately for housing and employment outcomes. Estimated using an OLS regression with robust standard errors ($n = 1,002$ for housing, $n = 1,002$ for employment). Vertical lines are 95 pct. (thin) and 90 pct. (thick) confidence intervals.

responsibility for either a positive outcome or a negative outcome, is actually more negative than neutral. The literature on the negativity bias thus suggests that negative information crowds out positive information (Olsen, 2015; Rozin and Royzman, 2001). If this is true, then it might make sense that the respondents assigned to the “neutral” condition respond in the same way as those assigned to the “negative” condition.

Validating the Results Using an Additional Survey Experiment

The results in the experiment above were, by and large, consistent with the self-serving bias hypothesis. However, do the findings actually reflect that voters are self-serving when it comes to assigning political responsibility? One might have two concerns in this regard. First, the differences in the way voters attribute responsibility for national vis-à-vis personal outcomes may reflect that voters have to operate on different ‘levels of analysis’. That is, the attributional differences might be borne out of voters dealing with aggregates (i.e., national house prices and national unemployment rate) rather than individual instances (the price of one house and the employment situation of one person). Second, voters might simply hold the government more responsible for negative outcomes that affect them personally than for positive outcomes that affect them personally even if these outcomes are completely outside the scope of the government’s control. That is, voters might be lashing out blindly when something happens to them personally.

To address these concerns, I ran an additional online survey experiment with 263 undergraduate political science students from a Danish university¹⁰. The survey included two experiments (presented sequentially), each meant to address one of the two concerns.

First, respondents were randomly split into two groups. The first two groups were randomly assigned to either the positive or the negative personal housing and employment outcome (cf. above). The other two groups received the same positive or negative outcomes, but with a small tweak. Instead of it being their own house or their own job, it was now “someone’s” job or “someone’s” house.¹¹ This is a more subtle difference than the national/personal split made in the main survey experiment, however, it should still regulate whether the first plausibility criteria is satisfied. As such, if voters are asked to evaluate who is responsible for someone

¹⁰The survey was conducted in February 2017 on the platform *psytoolkit*. The sample consisted of 55 pct. female students with an average age of 22.4 years.

¹¹The exact wording was “Imagine that someone in Denmark [lost their job/got a better job]” and “Imagine that the price of a Danish family’s house [increased/ decreased]”.

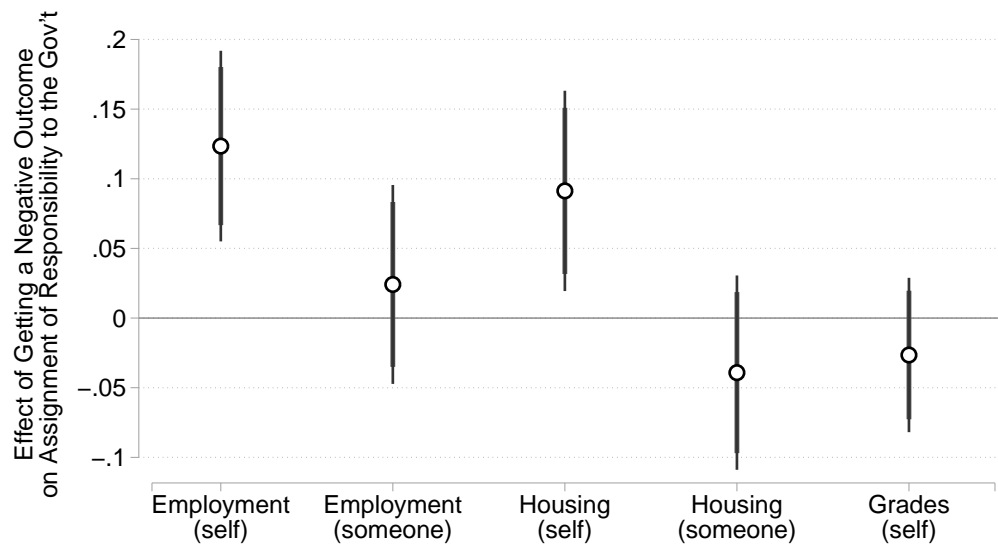


Figure 9.4 Effect of being presented with a negative rather than a positive outcome. Effects estimated using an OLS regression with robust standard errors. From left to right, the sample sizes used to estimate the effects are 141, 132, 143, 130, 263. Vertical lines are 95 pct. confidence intervals.

else's economic situation, then the self-serving motive should not influence voters' attributions, and there should be no valence asymmetry in attributions.

Second, respondents were presented with either a positive or a negative hypothetical personal outcome related to their own academic performance. In particular, they were presented with one of the following outcomes: "Imagine that you get a [bad/good] grade on an exam". This outcome is arguably completely outside of the government's control, and therefore we should not expect the self-serving bias to be relevant.¹²

After being presented with each of these different hypothetical outcomes, respondents were asked to evaluate the extent to which the government would be responsible for this outcome. I used the same question wording and scale as in the main experiment.

Figure 9.4 presents the effect of being presented with a negative outcome on how responsible the respondents thought the government was across the different outcomes. Three things stand out. First, the results from the main survey experiment replicate. Those who got a negative as opposed to a positive personal outcome were more likely to think the government was responsible across housing and employment

¹²In effect, the second plausibility criteria is not satisfied for this outcome.

outcomes. Second, there is no statistically significant difference for the housing and employment outcomes when voters were simply asked to imagine the outcomes happening to “someone” instead of themselves. Accordingly, it is not as though there is something special about small-scale outcomes. It is only when the small scale is the voters’ themselves, and self-serving motives thus become relevant, that there is a valence asymmetry in political attributions. Third, there is no difference between those assigned to the negative and the positive academic “Grades” outcome. This suggests that when outcomes are completely outside of the government’s control, there is no valence asymmetry in political attributions, consistent with the self-serving bias hypothesis.

All in all, the additional experiment increases our confidence in the self-serving bias hypothesis, as the results suggest that the conditional valence asymmetry in attributions identified in the main experiment is driven by voters making self-serving judgments.

Conclusion

A lot of research in social psychology have shown that people tend to exculpate themselves for undesirable outcomes yet implicate themselves in desirable outcomes. In this article, I have argued that this self-serving bias in attribution has important consequences for how voters attribute responsibility for policy outcomes. In particular, I have found that when responsibility for a policy outcome is shared between individual voters and their government, voters tend to shift blame towards the government when the outcome is undesirable and away from the government when the outcome is desirable. This type of valence asymmetry in attribution, however, is only present in settings where voters have a personal stake in the outcome.

I found evidence of such a self-serving bias in political attribution in a number of different places. For one, I showed that voters in Denmark, the US and Latin America are more prone to punishing their government if their personal economic conditions worsen than they are to rewarding their government if their personal economic conditions improve. This is a valence asymmetry which is not present when it comes to national economic conditions. I have also shown that there is a correlation between voters’ evaluations of their own personal economic situations and the extent to which they believe the government can influence their personal economic situations, but no correlation between voters’ evaluations of the national economic situations and their beliefs about whether the government can influence

the national economic situation. Finally, I have shown that if we ask voters to evaluate how responsible the government is for randomly assigned hypothetical outcomes, then, to the extent that voters have a personal stake in the outcomes, voters are less likely to hold the government responsible for desirable outcomes as opposed to undesirable outcomes.

These findings have important implications for existing work. The focus in the literature on how voters attribute political responsibility has gravitated towards attribution for events which are national in scope, like the national economy (Alcañiz and Hellwig, 2011; Duch and Stevenson, 2008) national emergencies (Healy, Kuo and Malhotra, 2014; Malhotra and Kuo, 2008) or how the government handles public service provision (Tilley and Hobolt, 2011; although see Tilley, Neundorff and Hobolt, 2017). Yet this study underscores the importance of also focusing on how voters attribute blame for outcomes which are more personal in nature (Ansell, 2014; Feldman, 1982; Giuliano and Spilimbergo, 2013; Kinder and Kiewiet, 1979). In particular, it seems that voters can potentially attach political significance to personal economic outcomes, although, as this study has shown, whether voters do so depend on the nature of these outcomes. This is a conditionality that might help explain why previous studies have struggled to pin down the exact importance of personal economic grievances (Kinder and Kiewiet, 1981; Lewis-Beck and Stegmaier, 2013; Nannestad and Paldam, 1994; Stubager et al., 2014).

Some caution is in order, however, before drawing broad implications from the results of this article. While the analysis covered a number of different countries and studied the self-serving bias using both observational and experimental data, the types of outcomes voters were asked to assign responsibility for were quite abstract. As such, it is unclear whether the self-serving bias would still be present if voters were faced by a concrete event, like actually becoming unemployed, where the amount of information about the causal process that led to this event is more dense. Furthermore, while the different empirical studies this article has undertaken do complement each other, they deal with potential methodological problems in sequential order rather than in tandem. As such, none of the studies provide a perfect test of the self-serving bias with strong internal *and* external validity. However, until such a test is conducted, we can tentatively conclude that there is a self-serving bias in attribution of political responsibility, which shapes how voters assign political responsibility in settings where it is unclear whether outcomes are a result of government intervention or individual's own behavior.

What implications does the self-serving bias in political attributions have for the incentives faced by reelection minded politicians? On the face of it, the self-serving bias should dissuade governments from pursuing policies which redistribute economic resources among voters. If voters who gain from economic redistribution do not credit the government for this gain, while voters who lose out blame the government, then redistribution is a lose-lose situation for governing politicians. This implication is similar to the one usually drawn from the literature on the negativity bias (e.g., Hood, 2010; Nielsen and Moynihan, 2017; Weaver, 1986). However, the fact that the self-serving bias is conditional on the plausibility criteria laid out above complicates thing a little. As such, the self-serving bias should only discourage politicians from pursuing re-distributive policies when these policies leave room for interpretation as to whether the government or the individual citizen is responsible for the outcomes this policy might make more or less likely. If there is any such ambiguity as to who is responsible, the findings from this article suggests that voters will seize upon it and attribute responsibility in a self-serving way. Accordingly, the self-serving bias might not dissuade politicians from spending money on direct cash transfers, where there is little ambiguity as to whether low-income beneficiaries are better off as a result of the policy, but it might dissuade politicians from investing in public services like education, where the low-income beneficiaries might rationalize that they would have been able to do just as well even in the absence of the public service. Accordingly, the self-serving bias in attribution of responsibility might have a detrimental effect on the provision of efficient redistributive public policies.

Supplementary materials

S1: Estimating the Valence Wsymmetry

The basic model used to look at how voters respond to economic conditions in study 1 is a linear probability model in line with this one:

$$Pr(y_{it} = 1) = \beta_0 + \beta_1 natwor_{it} + \beta_2 natbet_{it} + \beta_3 perwor_{it} + \beta_4 perbet_{it} + \epsilon_{it} \quad (9.1)$$

Here y is the dependent variable, support for the incumbent, $natwor$ and $natbet$ are dummies indicating whether the respondent believes the national economy is doing better or worse, $perwor$ and $perbet$ are dummies indicating whether the respondent believes their own economy is doing better or worse, and ϵ_{it} is the error term.

When we want to estimate the valence asymmetry we are really interested in the sum of the effects of the “better” and “worse” dummies. That is, we want to know how much negative effect is left once we take the positive effect into account. Based on equation one, the valence asymmetries can be described as $\beta_1 + \beta_2 = \theta_n$ and $\beta_3 + \beta_4 = \theta_p$. However θ_n , the valence asymmetry for the national economy, and θ_p , the valence asymmetry for the personal economy, are not estimated directly in model 9.1.

Instead, I estimate a slightly modified version of model 9.1. In particular, I incorporate θ_p and θ_n into the models by decomposing the “worse” effect into the valence asymmetry ($\theta_{p/n}$) and the “better” effect ($\beta_{4/2}$).

$$Pr(y_{it} = 1) = \beta_0 + (\theta_n - \beta_2)natwor_{it} + \beta_2 natbet_{it} + (\theta_p - \beta_4)perwor_{it} + \beta_4 perbet_{it} + \epsilon_{it} \quad (9.2)$$

This can be rearranged in the following way.

$$Pr(y_{it} = 1) = \beta_0 + \theta_n natwor_{it} + \beta_2 (natbet_{it} - natwor_{it}) + \theta_p perwor_{it} + \beta_4 (perbet_{it} - perwor_{it}) + \epsilon_{it} \quad (9.3)$$

This linear probability model includes θ_p and θ_n directly, and it can be estimated by creating new variables for national and personal economic perceptions which

subtracts the “worse” dummies from the “better” dummies. This is how I estimate the valence asymmetries which are plotted in figure 9.1.

S2: Surveys Included in Study 1

Election surveys from Denmark: 1990, 1994, 2001, 2005, 2007, 2011 and 2015. For details see <http://www.valgprojektet.dk/default.asp?l=eng>.

Election surveys from the US: 1980, 1984, 1988, 1992, 1996, 2000, 2004, 2008 and 2012. For details see: http://www.electionstudies.org/studypages/anes_timeseries_cdf/anes_timeseries_cdf.htm

Election surveys in the Latinobarómetro: The countries included in the Latinobarómetro, and the number of years that these countries have been part of the study, can be seen in table S.9.1. For details see: <http://www.latinobarometro.org/latContents.jsp>

Table S.9.1 List of included surveys from the Latinobarómetro

Country	First year	Last year
Argentina	1995	2010
Bolivia	1996	2010
Brazil	1995	2010
Chile	1995	2010
Colombia	1996	2010
Costa Rica	1996	2010
Dominican Republic	2004	2010
Ecuador	1996	2010
El Salvador	1996	2010
Guatemala	1996	2010
Honduras	1996	2010
Mexico	1995	2010
Nicaragua	1996	2010
Panama	1996	2010
Paraguay	1995	2010
Peru	1995	2010
Spain	1996	2010
Uruguay	1995	2010
Venezuela	1995	2010

S3: Asymmetry in Economic Experiences

In study 1, I find that there is a larger difference in support for the incumbent between those who think the economy is doing worse (as opposed to the same) than there is between those who think the economy is doing better (as opposed to the same). This is consistent with a self-serving bias in attribution of political responsibility. However, an alternative explanation is that the experience of those who are doing worse deviates more from the experience of those who are doing the same, than the experience of those who are doing better deviates from those who are doing the same. That is, the distribution of economic fortunes might be skewed, so that a lot of people experience something very bad and only a few people experience something very good. Could this explain the findings presented in study 1?

To find out, I revisit the Latinobarómetro data. I examine the surveys from after 2000, because these include a more detailed version of the question concerning voters' experience of the economy. In particular, voters could report their personal and the national economy as doing "a little" or "much" better or worse.

If there is a negative skew in economic experiences, we would expect the proportion answering "much worse" to be larger than the proportion answering "much better". I calculate these proportions for the question concerning voters' own, and the national economy, in table S.9.2.

Table S.9.2 Composition of economic experiences

	(1)	(2)	(3)	(4)
	Worse, own	Better, own	Worse, national	Better, national
Proportion extreme (i.e., 'much')	0.26	0.14	0.37	0.12
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	43990	51047	75053	41600

Standard errors in parentheses. Data from the Latinobarómetro.

As can be seen from table 2, there is some evidence of a negative skew in economic experiences: 14 pct. of respondents who thought their economy was doing better said it was doing much better, whereas 26 pct. of respondents who said their economy was doing worse said it was doing much worse. Even so, it seems unlikely that this can explain the valence asymmetry identified in study 1. As such, I found that the effect of the national economy was perfectly symmetrical, however, voters experience of the national economy was more asymmetrical than their experience of their personal economy. Twelve pct. of respondents who said the national economy was doing better said it was doing much better, whereas 37 pct. of respondents who said the national economy was doing worse said it was doing much worse.

In summary, there is some evidence that those experiencing a deteriorating economy are more likely to believe it is rapidly deteriorating, whereas those who experience an improving economy are more likely to believe that it is only improving a little. However, this asymmetry cannot explain the findings in study 1, because this experiential asymmetry applies to both national and personal economic conditions, whereas the attributional valence asymmetry identified in study 1 only applies to personal economic conditions.

S4: Tables Underlying the Different Figures

Tables S.9.3, S.9.4, S.9.5 present the OLS regression models used to produce figure 9.1.

Tables S.9.6, S.9.7 and S.9.8 present the OLS regression models used to produce figures 9.2, 9.3 and 9.4.

Table S.9.3 OLS regression of voting for presidential party (ANES)

	(1)	(2)
Worse off - own economy	-0.10* (0.01)	-0.10* (0.01)
Better off - own economy	0.06* (0.01)	0.06* (0.01)
Worse off - national economy	-0.26* (0.01)	-0.26* (0.01)
Better off - own economy	0.27* (0.01)	0.28* (0.01)
Female (ref: male)		0.04* (0.01)
Age		0.00 (0.00)
Some college or more (ref: none)		-0.02* (0.01)
σ	0.44	0.44
R ²	0.22	0.23
Observations	12,431	12,210

Standard errors in parentheses

Dummies for election survey omitted in model 1 and 2.

Ideology dummies and dummies for strength of partisanship omitted in model 2.

⁺ $p < 0.10$, * $p < 0.05$

Table S.9.4 OLS regression of voting for party in government (DNES)

	(1)	(2)
Worse off - own economy	-0.06* (0.01)	-0.04* (0.01)
Better off - own economy	0.00 (0.01)	0.02+ (0.01)
Worse off - national economy	-0.10* (0.01)	-0.11* (0.01)
Better off - own economy	0.12* (0.01)	0.12* (0.01)
Some college or more (ref: none)		-0.02* (0.01)
Female (ref:male)		0.02+ (0.01)
Age		0.00* (0.00)
σ	0.47	0.46
R ²	0.04	0.07
Observations	13,379	11,651

Standard errors in parentheses

Dummies for election survey omitted in model 1 and 2.

Ideology dummies and dummies for strength of partisanship omitted in model 2.

+ $p < 0.10$, * $p < 0.05$ **Table S.9.5** OLS regression of approving of incumbent president (Latinobar.)

	(1)	(2)
Worse off - own economy	-0.06* (0.00)	-0.06* (0.00)
Better off - own economy	0.04* (0.00)	0.04* (0.00)
Worse off - national economy	-0.13* (0.00)	-0.13* (0.00)
Better off - own economy	0.13* (0.00)	0.13* (0.00)
Female (ref:male)		0.00 (0.00)
Age		0.00* (0.00)
Some college or more (ref: none)		-0.04* (0.00)
σ	0.44	0.44
R ²	0.23	0.24
Observations	143,191	143,191

Standard errors in parentheses

Dummies for election survey omitted in model 1 and 2.

Ideology dummies and dummies for strength of partisanship omitted in model 2.

+ $p < 0.10$, * $p < 0.05$

Table S.9.6 OLS regression of beliefs about government's capacity to affect national and personal economic conditions

	(1) Personal	(2) National
Worse - Own	0.05 ⁺ (0.03)	0.04 ⁺ (0.02)
Better - Own	-0.03 (0.02)	0.00 (0.02)
Worse - National	0.05 (0.03)	0.06* (0.03)
Better - National	0.02 (0.02)	0.03 ⁺ (0.02)
Female (ref: male)	-0.03 ⁺ (0.02)	-0.05* (0.02)
Some college ore more (ref: none)	0.02 (0.02)	0.02 (0.02)
Age	-0.00* (0.00)	-0.00 (0.00)
Constant	0.78* (0.04)	0.78* (0.03)
σ	0.29	0.24
R ²	0.02	0.02
Observations	943	943

Standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$ **Table S.9.7** OLS regression of attribution to government

	(1) Employment Status	(2) House Prices
Worse - Own	0.04 (0.03)	-0.03 (0.03)
Better - Own	-0.16* (0.03)	-0.09* (0.03)
Neutral - National	0.16* (0.02)	-0.01 (0.03)
Worse - National	0.11* (0.02)	-0.05 ⁺ (0.03)
Better - National	0.17* (0.02)	-0.04 (0.03)
Constant	0.47* (0.02)	0.52* (0.02)
σ	0.23	0.24
R ²	0.19	0.01
Observations	1,002	1,002

Standard errors in parentheses. Reference category is "Neutral - Personal".

⁺ $p < 0.10$, * $p < 0.05$

Table S.9.8 OLS regression of respondents' attribution to government (additional experiment)

	(1) Employment (self)	(2) Employment (other)	(3) Housing (self)	(4) Housing (other)	(5) Grades
Negative Outcome	0.12* (0.03)	0.02 (0.04)	0.09* (0.04)	-0.04 (0.04)	-0.03 (0.03)
Constant	0.25* (0.05)	0.48* (0.06)	0.37* (0.06)	0.66* (0.16)	0.37* (0.05)
σ	0.20	0.21	0.21	0.20	0.23
R ²	0.09	0.00	0.04	0.01	0.00
Observations	140	132	141	130	263

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$

S5: Variable Descriptions for Economic Variables

The ANES use the following question with answers falling in one of three categories “better”, “worse”, and “the same”:

- Own: “We are interested in how people are getting along financially these days. Would you say that you and your family living here are better off or worse off financially than you were a year ago?”
- Would you say that over the past year the nation’s economy has gotten better, stayed about the same or gotten worse?

The Latinobarometro used two set of questions for the economic perceptions questions. From 1995-2000, the following questions were used:

- Country: “Do you consider the current economic situation of the country to be better, about the same, or worse than 12 months ago?”
- Own: “Do you consider your economic situation and that of your family to be better, about the same, or worse than 12 months ago?”

From 2001 and on the following questions were used:

- Country: “Do you consider the current economic situation of the country to be much better, a little better, about the same, a little worse, or much worse than 12 months ago?”
- Own: “Do you consider your economic situation and that of your family to be much better, a little better, about the same, a little worse, or much worse than 12 months ago?”

The DNES used the following questions, with answers falling in one of five categories: “better”, “a lot better”, “worse”, “a lot worse” and “the same”:

- Country: “How is the economic situation in Denmark today compared to one year ago?”
- Own: “How is your and your family’s economic situation today compared to one year ago?”

The DK-OPT survey used the following questions, with answers falling in one of five categories: “better”, “a lot better”, “worse”, “a lot worse” and “the same”:

- Country: “How is the economic situation in Denmark today compared to one year ago?”
- Own: “How is your and your family’s economic situation today compared to one year ago?”

S6: Descriptive Statistics

Table S.9.9 Descriptive statistics, DNES

	Mean	SD	Min	Median	Max	n
Vote for governing party	0.37	0.48	0.00	0.00	1.00	13379
Ideology (right-wing)	5.26	2.45	0.00	5.00	10.00	12841
State of personal economy	0.91	0.87	0.00	1.00	2.00	13379
State of country's economy	1.19	0.86	0.00	1.00	2.00	13379
Some college	0.32	0.47	0.00	0.00	1.00	13379
Age	48.69	16.85	17.00	48.00	106.00	13368
Woman (ref: man)	0.48	0.50	0.00	0.00	1.00	12089
Strength of party identification	0.79	0.86	0.00	1.00	2.00	13363

Table S.9.10 Descriptive statistics, Latinobarómetro

	Mean	SD	Min	Median	Max	n
Approve of president	0.52	0.50	0.00	1.00	1.00	143191
Ideology (right-wing)	25.68	38.69	0.00	6.00	99.00	143191
State of personal economy	0.85	0.86	0.00	1.00	2.00	143191
State of country's economy	0.89	0.77	0.00	1.00	2.00	143191
Some university	0.06	0.23	0.00	0.00	1.00	143191
Age	39.29	16.27	16.00	36.00	99.00	143191
Woman (ref: man)	0.51	0.50	0.00	1.00	1.00	143191
Strong feelings about president	50.55	49.22	0.00	99.00	99.00	143191

Table S.9.11 Descriptive statistics, ANES

	Mean	SD	Min	Median	Max	n
Vote for president	0.52	0.50	0.00	1.00	1.00	12431
Ideology (right-wing)	4.78	2.41	0.00	4.00	9.00	12431
State of personal economy	1.18	0.80	0.00	1.00	2.00	12431
State of country's economy	0.97	0.78	0.00	1.00	2.00	12431
Some college	0.63	0.48	0.00	1.00	1.00	12318
Age	49.13	16.77	17.00	49.00	93.00	12362
Woman (ref: man)	1.54	0.50	1.00	2.00	2.00	12431
Strength of party identification	3.04	0.96	1.00	3.00	4.00	12388

Table S.9.12 Descriptive statistics, DK-OPT survey

	Mean	SD	Min	Median	Max	n
Government responsible for national economy	0.77	0.24	0.00	0.75	1.00	943
Government responsible for respondent's economy	0.72	0.29	0.00	0.75	1.00	943
State of personal economy	2.11	0.60	1.00	2.00	3.00	943
State of country's economy	2.49	0.68	1.00	3.00	3.00	943
Some University	0.48	0.50	0.00	0.00	1.00	943
Age	52.68	17.49	19.00	55.00	97.00	943
Woman (ref: man)	0.48	0.50	0.00	0.00	1.00	943

Table S.9.13 Descriptive statistics, main experiment

	Mean	SD	Min	Median	Max	n
Government responsible for house prices	0.48	0.24	0.00	0.50	1.00	1002
Government responsible for employment status	0.52	0.26	0.00	0.50	1.00	1002
Age	52.83	16.66	18.00	55.00	88.00	1002
Man (ref: woman)	0.54	0.50	0.00	1.00	1.00	1002
Age	52.83	16.66	18.00	55.00	88.00	1002

Table S.9.14 Descriptive statistics, additional experiment

	Mean	SD	Min	Median	Max	n
Government responsible for housing (self)	0.50	0.22	0.10	0.50	1.10	141
Government responsible for employment (self)	0.43	0.21	0.10	0.40	1.00	140
Government responsible for employment (other)	0.51	0.21	0.10	0.50	1.10	132
Government responsible for housing (other)	0.49	0.20	0.10	0.50	1.00	130
Government responsible for grade	0.33	0.23	0.10	0.30	1.10	263
Age	22.37	3.02	18.00	22.00	44.00	262
Man (ref: woman)	0.46	0.50	0.00	0.00	1.00	263

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